BF
1
E87
V.31

BOSTON UNIVERSITY
COLLEGE OF LIBERAL ARTS
LIBRARY

22

THE AMERICAN
JOURNAL OF PSYCHOLOGY
Founded by G. Stanley Hall in 1887

Vol. XXXI JANUARY, 1920 No. 1

THE LOGIC OF THE NORMAL LAW OF ERROR IN MENTAL MEASUREMENT

By Edwin G. Boring, Clark University

No amount of practically successful "mental measurement" in laboratories, school-systems, factories or the army can relieve us, if we do not wish to waste time, of the necessity of stopping, every so often, to take account of first principles.

Psychophysics, with more than half a century of history to its credit, has repeatedly found the need to eliminate its logically unfit and reorganize its forces: it is a long cry from the principle of the just noticeable difference to the principle of the psychometric function. The mental test as a newcomer had first to prove its right to consideration. Now that it has been accepted it must pass under the critical eye and learn to conform. And what needs to be said, in way of admonition, applies especially to the mental test, although for no other reason than that the mental test is the lustiest form of mental measurement that one meets today. It, especially, merits a discriminating encouragement.

1. The Nature of Probability

We can get nowhere in an understanding of mental measurement unless we appreciate the relation of the unit of measurement to the frequency of occurrence of the different measures. In statistics we deal repeatedly with frequency-distributions; and we know, very often, the frequencies before we are assured as to what they are frequencies of. It is not unusual to attempt to calibrate a scale of measurement in terms of frequencies,—an attempt that always demonstrates the

necessity for a definite understanding of the relation between the two. Frequency-distributions, however, have a way of absorbing some of the mystic power that is commonly supposed to inhere in the normal law of error, the 'probability-curve,' which is itself a frequency-distribution. Into the mystery of this function, a mystery connected with its supposed a priori nature, we can pry only by studying its origins, logical and historical. On the logical side we must ask what, in scientific usage, is the nature of a probability. In such a beginning we are setting ourselves no mean task, but a necessary one if we are to avoid muddling.

The early history of the theory of probabilities is a history of the solution of problems arising in games of chance.[1] An applied mathematics of probability preceded the pure. For us the most significant principle that this period brought forth is one that is implicit in the famous theorem of James Bernoulli (1713).

This theorem holds that events tend to occur with relative frequencies proportional to their probabilities. If a is twice as likely as b to happen, then in the long run it will happen twice as often. But how, we may well ask, are we to know the probabilities of occurrence for a and b? We must exclude the inverse use of the Bernoullian theorem. We can not say that a is twice as likely to happen as b because it happens twice as often, for this is simply to reason from frequency to probability and ultimately to identify the two. Our problem was to predict frequency from given probability. How then do we achieve this given probability?

The case of the coin appears at first glance fairly obvious.

> If we may regard an ideal coin as a uniform, homogeneous circular disc, there is nothing which can tend to make it fall more often on the one side than on the other; we may expect, therefore, that in any long run of throws the coin will fall with either face uppermost an approximately equal number of times, or with, say, heads uppermost approximately half the times.[2]

Thus we read in a modern textbook of statistics. The thesis is: the probabilities are equal when there is no good reason for their being unequal. Von Kries has called this law the principle of insufficient reason ("*das Princip des man-*

[1] I. Todhunter, *History of the mathematical theory of probability*, 1865, is the standard historical work. He begins with Pascal and Fermat in 1654 and continues to Laplace: 624 pp.

[2] G. U. Yule, *Introduction to the theory of statistics*, 4th ed., 1917, 256.

gelnden Grundes");[3] Boole speaks of it as "the equal distribution of ignorance."[4]

Against an indiscriminate use of the principle of insufficient reason von Kries inveighs. We are asked to consider before the days of spectroscopic analysis the question of whether there is iron in Sirius. We are completely ignorant, but we can conceive of no greater reason for the presence of iron than against it. The probability for the existence of iron in Sirius is therefore ½, on the principle of insufficient reason. Similarly the probability for the existence of gold is ½. It follows algebraically that the probability for the existence of both iron and gold in Sirius is ¼, of iron without gold ¼, of gold without iron ¼, and of neither of these metals ¼. There were, when von Kries wrote, 68 known earthly elements. The probability of the existence of all these elements in Sirius is the 68th power of ½; *i. e.* the chance of all the elements existing in Sirius, as well as the chance of no one of them existing there, is about three in one-hundred-billion-billions. But if we had raised in the first place the question of the existence of earthly elements in Sirius, we might, being ignorant and applying the principle of insufficient reason, have said that it is just as likely that there are no earthly elements in Sirius as that there are some. The discrepancy between the three one-hundred-billion-billionths and one-half leads von Kries to declare that "die Aufstellung der gleich möglichen Fälle muss eine in zwingender Weise und ohne jede Willkür sich ergebende sein."[5]

Plainly it must. Shutting our eyes reduces our faith in what we see to less than one-half. If we would know the nature of our surroundings, we must open our eyes; and, if we would acquire a tenable belief of the chemical constitution of Sirius, we must await the spectroscope. There is no alchemy of probabilities that will change ignorance into knowledge. Expectations must be founded upon cogent rather than insufficient reasons. Nevertheless we can not, it would seem, push this antithesis too far. We can never be completely informed of all the conditions. Thus Westergaard:[6]

[3] Joh. von Kries, *Die Principien der Wahrscheinlichkeitsrechnung*, 1886, 6. The book is "eine logische Untersuchung." See also A. Kaufmann, *Theorie und Methoden der Statistik*, 1913, 43ff. C. Stumpf, in *Sitzungsber. d. k. bayr. Akad. zu München (philos.-philol. Cl.)*, 1892, 37-120, in arguing against von Kries, appears to defend the principle of insufficient reason; but in so doing he appeals to a wider analysis of the situation that is not wholly unlike a principle of cogent reason. See especially 41, 61-79.

[4] G. Boole, *An investigation of the laws of thought*, 1854, 370.

[5] *Op. cit.*, 10f.

[6] H. Westergaard, *Grundzüge der Theorie der Statistik*, 1890, 4.

> In every well-arranged game of chance . . . the balls are of the same size, of the same wood, of the same specific gravity, *etc.* They are mixed with the greatest care and every ball is apparently subject to the same forces. Yet such is not the case. In spite of all effort the balls are different, and depart, even though quite insignificantly, from spherical form. One approaches this and another that mathematical form. Every one has a weight and size different from all the others. No ball is absolutely like any of the others. Moreover they can not possibly lie in the bag in exactly the same manner. In short there are a multitude of apparently insignificant differences which determine that exactly this ball and none other shall be drawn.

In the face of such complexity it is hopeless to seek cogent reason for the drawing of the particular ball.

Moreoever, there is a logical as well as a practical reason for our inability to predict in the individual case. Westergaard seems to see this when he implies—it amounts to this—duplicity on our part when we arrange the game of chance. We attempt to make the balls all equal so as to equalize chance, we assume that we have succeeded for all practical intents and purposes, and yet in the same breath we expect to be able to draw some particular one of the balls. However, there must be inequalities, including of course difference of spatial position within the bag, in order to make the drawing possible.

> Did no such inequalities exist, then one of two things would happen: either all the balls would turn up at once or they would all remain in the bag.[7]

In more general terms, we may say that the problem of probability exists only in the face of ignorance. Given all the conditions, we deal not in probabilities but with the certainties of cause and effect.[8]

Here then it would seem we are landed in a dilemma. If we are in total ignorance of the conditions of our problem, all argument is foolishness. If we know all the conditions, we may argue to an effect but not to a probability. Have we to deal then either with a practical impossibility or with a logical absurdity?

The solution lies in the adoption of a definite intermediate position. Fisher lays down a first principle in the case:

> It thus appears that a rigorous application of the principle of cogent reason seems impossible. However, a compromise between this principle and that of the principle of insufficient reason may be effected by the following definition of equally possible cases, viz.: *Equally possible cases are such cases in which we, after an exhaustive analysis of the physical laws underlying the structure of the complex of causes*

[7] *Loc. cit.*

[8] Thus J. Venn, *Logic of chance*, 3rd ed., 1888, discusses probability as "amount of belief," where "ignorance of the individual is presupposed" and "causation . . . denied, within considerable limits:" p. 122.

influencing the special event, are led to assume that no particular case will occur in preference to any other.[9]

But Fisher, in attempting a compromise, neglects to tell us definitely how much ignorance and how much knowledge make the proper conditions from which we may proceed.[10] We are scientists engaged in very practical work and we wish to know what we may and what we may not do in our statistics.[11]

We can get at the nature of probability by taking a simple instance. The event in which we are interested is, let us say, the toss of a penny. We wish to know the probability that it will come heads. The law of insufficient reason says that, since we can not know that the penny is more likely or less likely to come heads than tails, the probability of heads is ½. The extreme demand for cogent reason would urge that, since we can not know the principal determining factors, this case is insoluble. The 'compromise' that Fisher proposes is that we shall analyze the underlying physical laws. We shall find that one side of the penny is heads and the other tails, that the penny is homogeneous, that it spins many times in the toss, and so on, until we are "led to assume" that neither heads nor tails will be preferred. Now the crux of the whole matter lies in the mechanism by which we are led to this assumption. In the case of the penny no knowledge of physical constitution and of the complexity of tossing would ever be valid ground, in itself, for believing that the penny would come heads as often as tails. In the initial instance this fact

---

[9] A. Fisher, *Mathematical theory of probabilities*, 1917, vol. I, 9. An interesting book, written with historical and philosophical perspective for practical purposes. It emphasizes the work of Continental statisticians in a way that English writers do not. Volume II seems not yet to be published.

The position is approximately Kaufmann's, *loc. cit.*, in distinguishing between *mangelnder* and *zwingender Grund*.

[10] It will not keep the statistician from error to tell him that his analysis must be "exhaustive" or that it must be so exhaustive as to 'lead him to assume . . .' Not only do investigators differ in natural thoroughness, but the problem may be restated, as we shall see, so that analysis of the properly known conditions may be pushed to the limits of cogency.

[11] The present paper in its inception is an attempt to answer certain questions that arose in the Office of the Surgeon General of the U. S. Army with respect to the ratings of recruits on intelligence tests. Actually logic is not a far cry from practice, but the technologist upon occasion can be deaf.

must be observed.[12] Thereafter we may reason that other homogeneous, one-headed, well-tossed pennies will in the long run, come heads fifty per cent of the time, and we may even go farther and reason that a homogeneous, six-faced, well-tossed die will in the long run come up equally often on every face. What we do is to assume probability on the grounds of observed frequency and its conditions.

Let us state the case in roughly syllogistic form.

The tossing of a penny gives, when indefinitely repeated, 50% heads;
The event in which we are interested is a tossing of a penny;
The event in which we are interested gives, when indefinitely repeated, 50% heads.

This leads us to predict what will happen to our particular penny, if it is tossed repeatedly; and, what is more important, it defines for us the place of cogent reason and the sphere of ignorance. We must know what a penny does when tossed again and again, either directly from observation or indirectly by inference from other observations. The major premise must be based upon cogent reason and there is no limit to the exactitude of the knowledge. Ideally the major premise may be considered to be indubitably established. Moreover we must know that the event in which we are interested is the toss of a penny in the sense of that term in the major premise. Our minor premise, by "an exhaustive analysis of the physical laws underlying the structure of the complex of causes influencing the special event," must also be established and without limit of accuracy by cogent reason. Of what then do we remain ignorant? Of the manner in which the particular, in the middle term, is subsumed under the general. In other words we know that our event is such an event that if repeated it will yield in the long run heads and tails equally, but we do not know whether this particular event, unrepeated, will be heads or tails.

Of an equal distribution of this ignorance, of a law of insufficient reason, we are saying nothing. But have we attained to a statement of probability by saying of a single event that

[12] "The assumption that any probability-constant about which we know nothing in particular is as likely to have one value as another is grounded upon the rough but solid experience that such constants do, as a matter of fact, as often have one value as another:" F. Y. Edgeworth, *Mind* O. S. 9, 1884, 230. K. Pearson approves this view: *Grammar of science*, 3rd ed., 1911, pt. i, 146. See also Edgeworth in article on Probability, *Ency. Brittanica*, 1911, xxii, 377, 391. There have been many attempts to supply "the rough but solid experience" empirically; *e. g.* Edgeworth, *Cam. Philos. Trans.* 20, 1905, 128ff.; Pearson, *Chances of death*, 1897, i, 13ff., 48; Westergaard, *op. cit.*, 21-56.

it is such that, if repeated, it would yield a given frequency of particular cases?

We can not within the narrow limits of this paper escape a certain amount of dogmatism. We must read widely elsewhere on the logic of chance. This much may, however, be said. It is plain that the probability of the event can not inhere in the event. The coming throw is either heads or tails, not half heads and half tails. An event is not even conditioned upon its probability. If, from a box of one thousand tickets, numbered consecutively, one draws any ticket at all, one obtains that particular ticket in the face of odds of 999 to one against it. The rule of such a drawing is that the improbable always happens. If the probability does not lie in the event, it must lie outside it; and we may here assert that it lies in the series to which the event belongs. When we ascribe a probability to a particular event we are simply seeing that event in a series in which the event is repeated as it varies in some particular phase; or, to put it more picturesquely, we see a series of repeated events telescoped within the single instance. This is our habitual mode of dealing with ignorance of this kind. Unable to prophesy the particular event, but able to prophesy its frequency, we content ourselves with the general in view of our impotence to deal with the particular.[13]

(And we may note in passing that ignorance has not bred knowledge in this process. Where reason was insufficient, prediction failed. We never can tell what will turn up next. We may be able to tell what will happen one way often and another way seldom, but that tells us not at all what the next one is).

[13] The paragraph is compatible with Venn's chapter on *Measurement of belief, op. cit.*, 119ff., *q. v.*, although divergent in emphasis. See also the discussion by F. M. Urban, Ueber den Begriff der matematischen Wahrscheinlichkeit, *Vierteljahrschrift f. wiss. Philos. u. Soziol.* 35, 1911, 1-49, 145-185. Only the margins of the logical problem are skirted here. Much might be said of subjective and objective probability. For the scientist-psychologist it is perhaps useful to distinguish four kinds of probability: (1) observed frequencies; (2) expected frequencies; (3) participation in a series of expected frequencies; and (4) psychological expectation. (1) is scientific fact; (2),derived from (1), is a prediction; (3) is a probability, a 'telescoped' (2); (4),the actually observable mental expectation, is a function of (3) and other conditions for determining tendencies, which the psychologist might do well to investigate. See Venn, 152-162. The metaphysical problem of the relation of physical constitution to frequency is beyond the point; actually the relation is observed. In more exact usage it would be necessary to assume that the phrase, 'indefinite repetition,' be taken to mean that the given frequency is approached as a limit when the repetitions are multiplied. The approach would not be continuous; Venn, 118.

We may seem far from the subject of mental measurement, but not without occasion, as we shall see. Let us turn now to the part played by the normal curve of error.

2. The Role of the Law of Error

The normal law of error[14] has been both an inspiration and a limitation in statistical measurement. Its formulation by Laplace resulted during the last century in a wide extension and application of the law to numerous forms of scientific, social and biological measurement; but Laplace is not responsible for all that occurred. There is a bit of magic in the formula. The law came to play the part of a first principle of nature, of an ideal, given *a priori*, to which nature seeks to conform. The mathematicians wrought slowly, but they wrought a god. Against such blind faith later statisticians have protested. They call the normal law a "fetish" and its *a priori* use a "superstition." Nevertheless the "superstition" still lingers and is mixed up with mental measurement. For this reason we are going to enquire, concerning the law of error, what real value it has for us to-day as a scientific tool.

In the days when the application of the theory of probabilities was largely confined to games of chance,[15] the logical difficulties discussed above had little opportunity to make trouble. The fact is that the cards in a pack, the faces of a die, and the sides of a coin do in the long run and under the conditions of gaming turn up about equally often. At this level common sense takes care of the theorist. It makes no practical difference whether the reason he assigns for the equality of occurrence is his lack of sufficient reason for any other result or the empirical fact that these things do work in such a way. Nor does it matter if unwittingly he applies to the single event the frequency that belongs to the series and calls it the probability of the event, for he knows by common sense that probability is not prophecy and that he

[14] The so-called "Gaussian" curve. The mathematical propadeutics for this function were prepared as long ago as the beginning of the 18th century (De Moivre, 1718). See Todhunter, *op. cit.* 71f., 136, 191ff., 552f. Laplace's *Theorie analytique des probabilités* brought together and supplemented in 1812 his work of thirty-five years, and gives the development of the law of error on pages 275ff. The proper date for the law is presumably 1786; see Todhunter, 485. Gauss gives the formula in Article 178 of the *Theoria motus corporum coelestium*, 1809, for which see his *Abhandlungen zur Methode der kleinsten Quadrate*, 1887, 102f. On his ascription of credit to Laplace, see *ibid.*, 207.

[15] The period covered by Todhunter, *op. cit.* Todhunter leaves off unfortunately where our most important period begins.

can not be assured of a realization of his expectations except in the long run. The fact is that the principle of insufficient reason and an indefinite conception of the nature of probability do when applied to games and modified by such unconscious reservations, furnish safe ground for practice, even though the logical implications are dubious.

Laplace and Gauss extended the theory of chance to the probability of errors.[16] Errors are a new material and it is not plain at once that the old assumptions hold. So far as the development of the law of error goes, these mathematicians were content to make their assumptions and reason abstractly to the general conclusion. Laplace worked in the tradition of the old school. He developed the law of error as the limiting case of the binomial expansion when the number of terms is infinite. This process is equivalent to assuming that, in a given case, an error is the resultant of a large number of sources of error, every one of which may affect the result in one way or the opposite (or affect it or not affect it), *i. e.* every source of error may, in the particular case, come up "heads" or "tails," as it were.[17] Either we have, in practice, to assume the principle of insufficient reason, which, applied to errors, does not have the sanction of common-sense experience; or we have to achieve an analysis of the case into sources of error and determine for every one that its law is the law of the coin, a course which is ordinarily impossible. Gauss hypothesizes explicitly that positive and negative errors are equally likely, and derives the law on this basis. Logically his assumption is the same as Laplace's, but it is usually stated in a form that suggests a test. If positive and negative errors are equally likely, then the arithmetical mean is the most probable value. To test the validity of the assumption in practice, we have then only to select a series (of observations, say, which are subject to error) and see whether the arithmetical mean occurs most frequently.

Laplace's derivation suggests the nature of error, Gauss' its mode of manifestation. Both these famous mathematicians were arguing from premise to conclusion when they derived the law of error, and science remains in their debt. We have no complaint to make until we find the law applied without an effort to establish the premises in the particular case. *The*

[16] *Cf.* W. S. Jevons, *Principles of science*, 1883, 375-385.

[17] Jevons, *op. cit.*, 380f. L. D. Weld, *Theory of errors and least squares*, 1916, 41-56, has an elementary discussion of the binomial expansion and the law of error. For the logic of this derivation applied to psychophysics, see E. G. Boring, *Am. J. Psychol.* 28, 1917, 465ff.

law will not work for errors, unless errors fulfill its conditions.

Gauss was clear about his assumptions, but he did not hesitate to apply the law to astronomical errors of observation.[18] Some of his followers, it seems, who had not witnessed the birth of the law, were less concerned in its application with its parentage. In theory of measurement the law was often accepted on its face value, in spite of the fact that a constantly increasing list of exceptions to it was being made out. In such cases the argument for the law seems to have been the practical one that it gave but little trouble in actual use. Thus Urban recently wrote:[19]

> We are confronted by the shocking situation that a proposition is triumphantly borne out by an immense indirect experience and that it can be proved neither by mathematical deduction nor by direct experience.

Urban, however, did attempt to prove it by showing, under the conditions of the psychophysical experiment, that positive and negative errors of observation, measured from the point of subjective equality, are equal.[20] Faith in the principle was established, however, long before psychophysics furnished this justification.

It is upon Quetelet, it seems, that we must fix the responsibility for the uncritical extension of the normal law to various human measures other than errors.[21] In making a man, he holds, Nature aims at an ideal, and the differences

[18] On the assumptions: Gauss, *op. cit.*, 4; on the applications: 92ff., 129, 139ff., *etc.*

[19] Urban, *Psychol. Rev.* 17, 1910, 242.

[20] *Op. cit.*, 240-244.

[21] An interest in the application of theory of probabilities to a variety of human problems was of long standing. Todhunter mentions the following initial attempts at application: expectation of life (Graunt, 1662), human testimony (Craig, 1699), human innocence (N. Bernoulli, 1709), birth-rates (Arbuthot, 1710-12), sex-ratios (De Moivre, 1718), astronomical observation (Simpson, 1757), marriages, smallpox, and inoculation (D. Bernoulli, 1760), weather forecasts (Lambert, 1773), annuities (Lagrange, 1798), modes of election (Laplace, 1812), and scientific observation in general (Laplace and Gauss). The important names are N. Bernoulli, De Moivre, D. Bernoulli, Lagrange, Trembley, and Laplace. See Todhunter, *op. cit.*, 37, 41, 54f., 193, 196, 211, 228f., 265, 320, 335, 349, 423, 426, 446, 500f., 546, 589-613, 615ff.

Laplace in the *Essai philosophique sur les probabilités*, which is a separately published introduction to the *Théorie* (*op. cit.*), devotes space to the application of probabilities to natural philosophy, to the moral sciences, to testimony, to elections in assemblies, to the judgments of tribunals, to tables of mortality, and to the duration of life, marriages, *etc.* Gauss is interested in applications to astronomy and geodesy. But all these things were more or less subject to a law of errors. Quetelet's conception of any natural variation as an error is

between men represent her degrees of error.[22] Quetelet refers for evidence to the measurement of circumferences of the chests of 5,738 Scotch soldiers and of the heights of 100,000 French conscripts. His position would be all very well were Quetelet content with supporting his hypothesis by this evidence. He seeks, however, to make it work both ways. He suggests that the deviation of the curve for the French conscripts from the normal law is due to fraudulent rejection for military service.

> This . . . law of continuity enables us to recognize a more remarkable fact: we might suspect it, but here we find it proved,—it is that the number of men rejected for deficiency in height is much exaggerated. Not only can we prove this, but we can determine the extent of the fraud. The official documents would make it appear that, of 100,000 men, 28,620 are of less height than 5 feet 2 inches: calculation gives only 26,345. Is it not a fair presumption, that the 2,275 men who constitute the difference of these numbers have been fraudulently rejected?[23]

Of course it is not a fair presumption, since the law depends on the official documents for confirmation. Nevertheless, while admitting the dependence of the law on experience, Quetelet proceeds in numerous cases to analyze experience by means of it. Such a double-edged sword is a peculiarly effective weapon, and it is no wonder that subsequent investigators were tempted to use it in spite of the necessary rules of scientific warfare.

Galton in the *Hereditary Genius* applied the normal law to

---

an undoubted extension of the principle, although it is difficult to draw a line.

A. Quetelet was a Belgian statistician, who published an *Essai de physique sociale* in 1835 and a collection of *Lettres sur la théorie des probabilités* in 1846. He writes in the tradition of Laplace with little reference to Gauss. That Gauss' work on least squares was of interest to the French school is evidenced by its translation into French (from the Latin) in 1855, thirty-two years before a similar publication in the German language.

There is no question as to Quetelet's responsibility for the extension of the principle: *cf., e. g.,* W. Lexis, *Zur Theorie der Massenerscheinungen in der menschlichen Gesellschaft,* 1877, 11; Kaufmann, *op. cit.,* 12, 163.

[22] *Letters on the theory of probabilities,* trans. 1849, 91-96. "The difference which Nature makes in the heights of men is not greater than that which inexperience would produce in the measurements taken on one individual man in an attitude more or less curved." "Everything occurs as if there existed a type of man from which all other men differed more or less." "Each people presents its mean, and the different variations from this mean, in numbers which may be calculated *a priori.*" (pp. 95f.)

Cf. the discussion and criticism of "l'homme moyenne" in Kaufmann, *op. cit.,* 110ff.

[23] *Letters,* pp. 97f.

mental differences and, using it *a priori*, worked from frequencies of natural ability to a scale of equal intervals of ability. He frankly accepted "the very curious law of 'deviation from an average'" from Quetelet, showed that certain mental capacities obeyed the law approximately, and then argued from the law to "grades of natural ability, separated by equal intervals."[24]

The English school of biometricians, which derives directly from Galton, has furthered the *a priori* use of the normal law that obtains to-day in much biometric and mental measurement.[25] There is an early study by Pearson that illustrates the point.[26] Weldon had measured the lengths and breadths of two large samplings of crabs: one set from Naples and one from Plymouth. The Plymouth batch gave symmetrical distributions, but the frequency curve for the breadths of the Naples batch was skewed. Pearson, by an elaborate mathematical treatment, analysed the skew curve into the sum of two normal curves, the averages of which did not coincide, and Weldon accepted this result as evidence of "dimorphism." Pearson then applied his method to a symmetrical curve for the Plymouth crabs and found that there was no real solution. He assumed, therefore, that these data were "homogeneous." Further he laid down the general rule that some such analysis must be attempted upon every apparently normal curve before a conclusion can be reached as to the homogeneity of the data. If the actual results can be better represented by the sum of two normal curves than by any single normal curve, then there is heterogeneity. Even curves that approximate the normal very closely must be tested, for they may yield to analysis.

Thus Pearson showed that the distribution of shots at a

[24] F. Galton, *Hereditary Genius*, 1869, 26-36. G. Th. Fechner, of course, had already used the law in the method of right and wrong cases with sensory material. He seems not to have hesitated in his acceptance of the law from Gauss: *Elemente der Psychophysik*, i, 1859, 104. He applied it, not to sensations, but to the *scheinbare Reizgrösse*, the effective inner stimulus: *Revision der Hauptpuncte der Psychophysik*, 1882, 40f., 45. Current notions of "deviation from an average" come undoubtedly, however, from Galton and Quetelet.

[25] See, for example, M. R. Trabue, *Completion-test language scales*, 1916, 29-60, where the normal law is assumed in order to obtain a scale of equal units. Trabue is quite explicit about the assumption (see note, p. 31), bat he evidently believes it is correct or he would not devote so much space to developing it.

[26] K. Pearson, Contributions to the mathematical theory of evolution, *Philos. Trans.* 185A, i, 1894, 71-110. The basal data are given by W. F. R. Weldon, *Proc. Roy. Soc.* 54, 1893, 318-329.

target, given by Merriman,[27] is best represented by the sum of two normal curves. The gun must be thought of as aiming, not at the sixth band of the target as Merriman supposed, but at the fifth and seventh bands (explainable perhaps by a change of sighting during mid-firing). Here then, we find no attempt at all to seek empirical support for the normal law. The crabs were obtained in a single selection and the shots were made in a single experiment, and each set appears, even after it is charted in a graph, as if all the measures belonged together. But we apply the criterion of the normal law and we find the crabs dimorphic and the gun double-sighted.[28]

Now the very remarkable thing about this easy acceptance of the law of error as the rule of nature is that it has been done all along in spite of very much better available information. Let us get some idea of what has been said.

We saw that Laplace's assumptions were implicit in his method and Gauss' explicit in his text. We had no quarrel with them, but we blamed Quetelet. Did Quetelet's thesis pass, then, quite unchallenged? No, for in the notes to the *Letters* he printed three letters from Bravais,[29] who presented figures on the skew distribution of barometric pressures, discussed various reasons for departures from the normal law, and said in brief:

> I think that generally every partial and distinct cause of error gives place to a curve of possibility of errors (or, if preferred, of differences about the mean), which may have any form whatever,—a curve which we may either be able or unable to discover, and which in the first case may be determined by consideration *a priori* on the peculiar nature of this cause, or may be determined *a posteriori* by observations on the isolated condition of other concomitant causes of error.[30]

[27] M. Merriman, *Textbook of the method of least squares*, 1884, 14. These data have frequently been quoted as illustrating the application of the normal law to errors.

[28] Of course, if we wish to define "heterogeneity" solely by reference to the applicability of the normal law, nothing can be said, though the usefulness of the procedure might be questioned. The almost unavoidable implication of such a course, however, is exactly that which Weldon got from it, *viz.* that the two normal components point to a biologically significant dimorphism. That idea, moreover, originated with Quetelet: *Letters*, 96f. On the other hand, it is only fair to Pearson to point out that the theory of skew variation in homogeneous material must already have been taking shape; see his distinction in the year following between asymmetry due to heterogeneity and asymmetry due to the mode of variation of a homogeneous material: *Philos. Trans.* 1862, i, 1895, 344f.

[29] Quetelet, *Letters*, 286-295. Venn, *Nature*, 36, 1887, 411f., shows the same asymmetry of barometric heights that Bravais pointed out.

[30] P. 290.

In introducing the letters from Bravais, Quetelet remarked:

> In this memoir I only had in view the examination of the case in which accidental causes have had no tendency to act in one direction more than another.[31]

We have already shown that Quetelet actually established the law by the data and then corrected the data by the law.

In 1869 Galton accepted the law of deviation from an average as applicable to mental ability, but in 1879 he was ready to insist on exceptions:[32]

> My purpose is to show that the assumption which lies at the basis of the well known law of "Frequency of Error" . . . is incorrect in many groups of vital and social phenomena, although that law has been applied to them by statisticians with partial success and corresponding convenience. . . .
>
> The assumption to which I refer is, that errors in excess or deficiency of the truth are equally probable; or conversely, that if two fallible measurements have been made of the same object, their arithmetical mean is more likely to be the true measurement than any other quantity that can be named. . . .
>
> Suppose we endeavor to match a tint: Fechner's law, in its approximative and simplest form of sensation=log stimulus, tells us that a series of tints, in which the quantities of white scattered on a black ground are as 1, 2, 4, 8, 16, 32, &c., will appear to the eye to be separated by equal intervals of tint. Therefore, in matching a grey that contains 8 portions of white, we are just as likely to err by selecting one that has 16 portions as one that has 4 portions. In the first case there would be an error in excess, of 8; in the second there would be an error in deficiency, of 4. Therefore, an error of the same magnitude in excess or in deficiency is not equally probable.

Thus Galton argues that the geometric mean is the most probable value with sensory material or data that follow a similar law. The distribution curve in such cases would be markedly skew.

We remarked that Pearson in 1894 gave evidence of being influenced by the sanctity of the normal law. His faith was not great for he noted its limitations in 1895.[33] In 1900 the faith was gone.[34]

> It appears to me that, if the earlier writers on probability had not proceeded so entirely from the mathematical standpoint, but had endeavored first to classify experience in deviations from the average, and then to obtain some measure of the actual goodness of fit provided by the normal curve, that curve would never have obtained its present position in the theory of errors. Even today there are those who regard it as a sort of fetish; and while admitting it to be at fault

[31] P. 286.

[32] *Proc. Roy. Soc.* 29, 1879, 365-367. The paper is followed by a discussion by D. McAlister, *ibid.*, 367-376 which gives the mathematics and presents some graphs of skew distributions that would result from the logarithmic scale of measurement.

[33] *Philos. Trans., loc. cit.*

[34] *Philos. Mag.*, 5th ser., 50, 1900, 173.

as a means of generally describing the distribution of variation of a quantity x from its mean, assert that there must be some unknown quantity z of which x is an unknown function, and that z really obeys the normal law! This might be reasonable if there were but a few exceptions to this normal law of error; but the difficulty is to find even the few variables which obey it, and these few are not usually cited as illustrations by the writers on the subject.[35]

Pearson in developing a system of skew curves whereby he seeks to adjust theory to nature has performed great scientific service in combatting the superstition of the normal law.[36] The paper of 1894 suffered on account of its occasion, *viz.*, the problem of dimorphism in Weldon's crabs. But not all investigators have avoided initial error; and presently we must return again to Pearson.

These quotations from Galton and Pearson suggest a reason why the normal curve should be of exceptional occurrence. If "log stimulus" gives a normal curve, says Galton, then "stimulus" will not. If x does not give a normal curve, some persons, says Pearson, assert that z, of which x is a function, does follow the law. Suppose then that nature does conform to the normal law; we shall not in practice be fortunate enough to obtain normal curves of distribution unless we have chanced to use nature's own unit of measurement, or some unit directly proportional to it. Bertrand pointed this out long ago.[37]

La règle des moyennes, il importe d'insister sur ce point, n'est ni démontrée ni exacte. S'il était admis que la moyenne entre plusieurs mesures fût toujours la valeur la plus probable, il en résulterait des contradictions. Quand on mesure une grandeur, on mesure, par cela même, toutes les fonctions de cete grandeur, son carré par exemple, ou le logarithme du nombre qui la représente. Pourquoi la valeur la plus probable du carré ne serait-elle pas la moyenne des valeurs obtenues pour le carré, et la valeur probable du logarithme, la moyenne des logarithmes? . . .

Il ne faut pas, pour écarter l'objection, faire une distinction entre les grandeurs directement mesurées et celles qui résultent d'un calcul. Un mécanicien pourrait, bien aisément, annexer à une balance une aiguille marquant le carré ou le logarithme du poids. Ce carré ou ce logarithme deviendrait alors la grandeur mesurée. Le postulatum admis dans un cas devient donc impossible dans les autres.

A better example would be the distribution in size of the cubical crystals of common salt. In a sample of such crystals we might measure size by the height of the cube or by its

[35] The remarks are occasioned by Merriman and the target-shooting. Merriman has subsumed these data under the "fetish"; but we have seen that it is not perfectly clear why Pearson in 1894 subsumed the same data by a two-fold application of the same "fetish."

[36] The early papers on "Skew variation in homogeneous material" are *Philos, Trans.* 186A, i, 1895, 343-414, and 197A, 1901, 443-459. See also "On the general theory of skew correlation and non-linear regression," *Drapers' Co. Research Memoir*, 1905.

[37] J. Bertrand, *Calcul des probabilités*, 1889, 180f.

weight. Since the latter measure should be approximately proportional to the cube of the former, it is plain that the normal law could not hold in both cases.[38] Now, if nature seeks to conform to the normal law, does she prefer to conform in lengths or in volumes or, perhaps, in some other more "natural" unit? At least, if man would seek the law of nature, he must have cogent reason for his choice of measures.[39] He can not (as the behavorist would agree!) tell what nature is doing unless he knows what nature is trying to do.

We have been speaking of France and England, but the reaction against the "Gaussian dogma" was more explicit in Germany. In 1877 Lexis published a *Theorie der Massenerscheinungen*[40] in which frequency distributions were dealt with as normal, subnormal, or supernormal.[41] The emphasis was thus shifted from the normal distribution to normal deviations from the normal distribution.[42] The "Lexian ratio" measures this deviation.[43] This point of view has met with acceptance among German and Scandinavian actuarial statisticians, and forms, for example, the basis of Czuber's *Wahrscheinlichkeitsrechnung*.[44] We have already seen that von Kries in 1886 did not include among valid logical principles the law of insufficient reason. He was inclined to believe, however, that in many cases the "Gaussian law" was realized,[45] and he cites in support Bessel's observations, which Urban refers to as an example of asymmetry.[46]

Westergaard in 1890 sums up the empirical case.[47] He

[38] It would be interesting to test this relation in the case of heights and weights of men. Height-and-weight tables are available in anthropometrical and somatological texts.

[39] We have a cogent reason, presumably, when, with the Fechner pendulum, we take as the unit, not the angle read on the scale, but the square of the sine of half that angle.

[40] Lexis, *op. cit.*; see especially 34ff.

[41] Fisher's translation of *unternormal* and *übernormal* as "subnormal" and "hypernormal" is due presumably to his otherwise excellent "dano-English," for which he so modestly apologizes: *op. cit.*, 125 and xv.

[42] If a pun is permissible, for Lexis' thesis is that the "normal curve" is not "normal" in the sense "usual."

[43] *V.* Fisher, *op. cit.*, 124ff.

[44] E. Czuber, *Wahrscheinlichkeitsrechnung*, 2 Aufl., 1910, 34-78.

[45] *Op. cit.*, 226ff.

[46] *Op. cit.*, 241. Much of the dispute as to whether the normal law is realized in fact, depends on just how much departure of fact from theory we allow without giving up the theory. Von Kries could call normal in 1886 what Urban would see as asymmetrical in 1910: standards have gotten more rigorous.

[47] *Op. cit.* 10-83.

brings together the experiments on the drawing of balls and the like, and shows that with increasing number of cases the relative frequencies lie within a decreasing *Spielraum* and thus in a sense approach the expected frequencies. He then goes on to show that the frequencies of social statistics behave in a manner similar to the frequencies of the experiments on "chance." He is not so much interested in the applicability of the normal law as he is in the facts. The appeal to facts is, however, always a protest against theory which is given *a priori*.

After all the facts are the scientific business. We have traced what seemed to be a growing realization of the logical inadequacies of a theory, but have we perhaps not been witnessing a deepening appreciation of the facts? When anthropometric measurements were first being made, there stood out the fundamental fact of the massing of cases about the average, the rapid falling off of more extreme frequencies, and the extreme rarity of widely divergent cases. To this extent the normal law was the fact. But as interest centered upon the details, the inadequacies of so simple a generalization became apparent. There was effort enough at reconciliation, but, in general, science kept to the facts, and a more flexible system of representations came into use.

There is a pretty instance of the inadequacy of the normal law to scientific description in a research of Pearl's.[48] Pearl studied statistically the number of leaves in a whorl in Ceratophyllum. He found that the distributions for number of leaves were skewed one way at the proximal end of the main stem or of a branch, the other way at the distal end, and that the form of the distribution passed through symmetry somewhere between the two ends. Thus he writes:

> The phenomenon of skew variation stands forth in this case, free of doubtful interpretation through selection or any similar factor, clearly and definitely as a phenomenon of growth. In the face of facts of this kind it is difficult to understand how anyone can be so firmly convinced of the *Allgemeingültigkeit* of the normal or Gaussian law, as some biologists still are . . . Skewness in variation is a very real biological phenomenon, which may be changed and modified, not only in degree, but in direction, by various biological factors like growth, as, for example, in the present case.

In other words, if nature is aiming to make something follow the normal law, then, as the stem of Ceratophyllum grows, she changes continuously that something which is to fit the law. The real problem of scientific description is an account

[48] R. Pearl, *Variation and differentiation in Ceratophyllum*, 1907; especially 90f.

of nature's changing. To assume the normal law in such a case would be to shut one's eyes to the fact.

Here we may leave the question of the *a priori* nature of the normal law. There is, after all, no magic in it. It gives us back always what we put into it. If we know from experience what nature is up to, as we do with the coin, then we can proceed upon cogent reasons to apply the law and we get results. If we do not know, we must appeal to nature and see. We have then no reason to expect that we are going to find the law before the appeal is made, whereas we have considerable reason to expect not to find it, since the form of distribution depends on the unit of measurement and we have arbitrarily chosen one of a possible infinite number of units. When we do go to nature we find all degrees of resemblance to the law and divergence from it, and we may even find that the degree of divergence from the normal becomes the significant fact of our observation.[49]

3. The Relation Between the Unit of Measurement and the Form of Distribution

We have seen that if we alter the unit of measurement we alter the form of the frequency-distribution, and that if the change is in accordance with any function other than direct proportionality we get alterations of the skewness of the curve. Bertrand made the point with especial clearness, and it is strange that a relation so obvious should have received so little attention.

Williams recently ran upon the difficulty when experimentally he obtained psychometric functions for memory.[50] He

[49] We have noted that the universality of the normal law was contested by the following persons: Bravais, 1845; Quetelet (admitted Bravais' argument), 1846; Lexis, 1877; Galton, 1879; McAlister, 1879; Bertrand, 1889; Pearson, 1895 and emphatically in 1900; Pearl, 1904; Urban, 1910; Edgeworth, limitedly in the *Britannica*. The list is by no means complete. We may add: R. L. Ellis, *Philos Mag.*, 3 ser., 1850, 321-328; Jevons, *op. cit.*, 1883, 374ff.; Venn, *Nature*, 36, 1887, 411ff. and *Logic of chance*, 1888, 23-52, 501ff.; Fisher, *op. cit.*, 1917, 149. Urban, *op. cit.*, 241, mentions Helmert, Laurent, Bessel, Guarducci, and Edgeworth as demonstrating asymmetry of distribution. Fisher, *loc. cit.*, refers to Oppermann, Cournot, and Dormay (especially Dormay) as showing the inadequacy of the Gaussian dogma. Urban's serious consideration of Pierce's data as showing symmetry is hard to understand for Pierce's symmetry was presumably assumed in the first place. His beautiful curves must have been fitted, for all he says of them, by eye, and when one attempts to treat his data statistically they do not check. *E. g.*, the numbers of cases for days 20 to 24 are respectively 495, 494, 502, 499, 498, and 497. Each should be 500. C. S. Pierce, *Report of U. S. Coast Survey*, 1870, 212ff.

[50] H. D. Williams, *Amer. J. Psychol.* 29, 1918, 219-226. See also Boring, *Psychol. Bull.* 15, 1918, 32f.

expressed the frequencies for recall of a material as a function of the number of repetitions of the material, but he felt, for certain cogent reasons that the student of memory will appreciate, that the repetition was not a satisfactory unit in terms of which to work. The psychophysical effect of an early repetition is presumably greater than that of a late one and Williams would have liked a more truly mnemonic unit. If he could have taken the normal curve *a priori* as the necessary function, he could have adjusted his results to it and have discovered a mnemonic unit that was some particular function of the number of repetitions. Such a relationship would have provided a knowledge of the effectiveness of successive repetitions and would have produced a method for use with other mnemonic measures. All measures eventually could have been interrelated, and a great advance made in the psychophysics of memory. Unfortunately, however, there was no justification for assuming the normal law, and, with both law and psychophysical unit unknown, Williams, as he realized, was placed in the dilemma of being able to reason from neither to the other. The way out, of course, was to give up the impossible, and to stick to the observed facts, leaving the frequencies to stand as a function of the number of repetitions.

Trabue, in attempting to establish a language-scale "with equal distances between steps," ran into the same difficulty.[51] His material,— sentences to be completed by the Ebbinghaus method,—had no quantitative aspect except the number of arbitrarily chosen completion-tasks that could be successfully accomplished. He chose, therefore, to assume the normal law and to calibrate his material by reference to it, taking the probable error as the unit.[52] His hint of a cogent reason back of the normal law stands quite unsupported.[53] Now the language-scales presumably do "work;" but if they do, it must be for the reason that this form of measurement, pretending to nothing more accurate than Quetelet achieved, counts his degree of rigor successful.

[51] See foot-note above, p. 12.

[52] Pp. 30ff. There is a further, somewhat obscured assumption, *viz.*, that the P. E. for one age equals that for another: see pp. 54ff. That the dispersion would not alter with age would hardly be expected; *cf.* J. B. Miner, *Deficiency and delinquency*, 1918, 252ff. At any rate the assumption is gratuitous, unless, as is always true, we argue that the unit is the premise in the case, and that premises are indisputable. The law of insufficient reason is usually applied to personal predilection.

[53] A foot-note on p. 31 says of the normal law: "If this assumption is made the results that follow are in . . . close accord with known facts;" but there is nothing more.

These two papers are but examples of how the problem of the unit arises in very different fields of mental measurement and how their solution is balked by an inability to assume in advance some distributive function like the normal law.[54] In such cases the limitation is pretty clear, for we have to make an assumption and it takes but little argument to show that an assumption in itself can not be fact.

There is, however, a more subtle way in which the will to believe in the normal law gets in its work. Suppose we give up assumption and go to experience, in order to study a special group of related phenomena; and suppose we find that very many of these phenomena, as we measure them, are distributed every one in accordance with the normal law? May we not conclude by analogy that the same law applies to other phenomena? If a penny comes heads and tails equally often, will not a silver dollar? If stature follows the normal law, what about length of forearm?

The answer is that it all depends on how much one knows of the nature of the case. One may reason from the penny to the dollar, if one knows — inductively, from experience — that homogeneity and shape in the penny are the attributes that condition the law, whereas size, chemical constitution, color, *etc.*, are irrelevant. (Even so one may make a mistake; a magnetized steel disk in a magnetic field might give unexpected frequencies.) To reason from stature to length of forearm, however, requires that we know that the size of the forearm is conditioned in the same way as is the total length of the body. It is not enough to know that both fore-

[54] There is no need to assemble a list of studies where the error in question is committed. Often it is present only by implication. Pearson, for example, in his study of "The relationship of intelligence to size and shape of head, and to other physical and mental characters," *Biometrika* 5, 1906, 105-146, assumes the normal curve in order to provide a scale on which his results may be plotted. He is careful to point out that the assumption is merely for convenience and that it does not affect the chief business of the paper, *viz.*, the computation of correlation ratios (pp. 106f., 118). Very well. But then Pearson proceeds to set up a scale of units called "mentaces," with every unit 1/100 parts of a particular group that about equals the standard deviation. A complete scale of mentaces is suggested, ranging from −300 to +300. Offhand it looks like measurement, although the careful reader discovers that it is merely an illusion resulting from overindulgence in the normal law.

Miner's discussion, *op. cit.*, 254-276, is here to the point. Miner sees the problem of the unit (255f.) and combats the Gaussian superstition (267ff., 272-276). His strictures on Pearson (272ff.) are tolerant. He does not seem, however, fully to appreciate (in spite of pp. 255 and 273f.) that the problem of the unit and of the form of distribution are one and the same.

arm and stature depend on the same factors; we must know that they depend upon them in the same way, that is to say, we must know that we are dealing with the same scale of growth. If the forearm were to vary as the square of x and stature as the cube of x, then prediction that the distribution of the one would be like the distribution of the other would be doomed to disappointment. With the coins, we know that the scales are comparable; each is expressed in percentage of heads. But in biological and psychological phenomena we can scarcely hope for any such certain analysis of the essential conditions.

Apparently then sometimes we may generalize and sometimes not. Can we make out a list of the classes of phenomena to which it is safe to say that the law applies? At least we may try.[55] And we shall have to consider five classes.

1. In the first place there are the *Glückspiele,*— coin tossing, card drawing, dice throwing, roulette, and the persistent urn or bag with the red and white balls inside, that introduces all students to the theory of probabilities. In such cases the presumption for the validity of the law is stronger than anywhere else. It seems that the red and white balls ordinarily do come out of the bag about equally often if they exist in equal quantities inside. Westergaard in 10,000 drawings got 5,011 white balls. Yet Pearson found that Weldon's 26,306 throws of dice exhibited "a bias toward the higher points." He calculated that the chances of 5 or 6 coming up on these dice exactly one-third of the time were less than two in 100,000, but that the chances of 5 or 6 coming up .3377 of the time

[55] What is needed is a comprehensive statistical study. If some one would get together only a hundred distributions based on large numbers (physical, biological, and mental distributions as found in nature, and distributions from such things as games of chance, tide tables, incommensurable numbers, *etc.*, as found in human civilization), and if he would treat them all accurately *by the same method*, applying some criterion of goodness of fit to the normal law (*cf.* Pearson, *Phil. Mag., op. cit.*), we should begin to have some notion of the way in which all these things do behave. But the inexperienced statistician should not begin lightly on such a task. He must be prepared to use much judgment before results taken under very different conditions are comparable. The very decision as to the size of the class-interval in every case is tremendously baffling; and there are questions of weighting that are equally bothersome. When this task is done he ought to fractionate and do it all over, in order to see how the principle of the *Spielraum* as dependent on the law of large numbers works out. And after that, he has left the study of the effect of changes in the scale of units. How does the goodness of fit alter when the logarithm or the square root of the measure is substituted for the measure, and how much does it alter and under what conditions does it alter most? It is a large order and one that is well worth filling.

were as great as two in 16. "Oh well," we say, "you could not expect any real dice to be exactly homogeneous. There is not much difference between .3377 and ⅓." No, and in any real experiment ideal conditions are never exactly realized. All experimental observation is only as accurate as the control of the conditions of observation.[56] The tendency with Pearson's method is to conclude from particular cases against a generality by a method more precise than the precision of the observed cases allows.[57] In general the *Glückspiele* seem to follow the law as well as the faulty conditions of their construction and operation permit.[58]

2. It is generally maintained that the occurrence of digits in the expansion of an incommensurable number like π. ϵ. $\sqrt{2}$, log 7, *etc.* follows the normal law, and that all digits in the long run occur equally often. Here the law of insufficient reason is peculiarly tempting. If there is no relation at all between the intrinsic properties of a circle and the number of fingers on the two human hands which determined the decimal system, then why should not all digits turn up equally often in the expansion of π? There is no reason why they *should* not. The *fact* is that in the first 707 digits of the decimal of π the sevens occur only three-fourths as often as they should, although the other digits do better by the law.[59] This case, however, is but a single one,[60] and, as Venn points out, the chances of one digit's deviating from equality as much as do

[56] Of course we *want* the die to come up equally often on every face. When it does not, we would rather blame the die than the law. But, though such a gaming 'instinct' may be built into human nature, it is nevertheless an insufficient reason for science. To blame the die because it does not fit the law is unfair; but to say that the die because of practical conditions does not disprove the law or even that it does approximately prove it, is allowable.

[57] But this is a paper in itself.

[58] For summaries of results, see Westergaard, *op. cit.*, 21-38; Pearson, *Chances of Death*, 1897, i, 11-25, 44-62. The failure of Monte Carlo to obey the law of chance seems not to apply to all roulette; *v.* pp. 57f. Westergaard's experiment is in *op. cit.*, 36f.; Pearson's computations are in *Phil. Mag.*, 5 ser., 50, 1900, 167ff. See also Fisher, *op. cit.*, 127-145.

[59] The actual frequencies, which Venn who discusses them (see note below) does not give, are: 0, 73; 1, 78; 2, 72; 3, 74; 4, 74; 5, 63; 6, 68; 7, 53; 8, 73; and 9, 79. The expansion of is by W. Shanks, *Proc. Roy. Soc.* 21, 1873, 319.

[60] There is no reason why a division of a decade by a trinity should be prepotent of other trinities, that 10 divided by 3 should be 3.33333 . . . (in the decimal system; the duodecimal system gives a completed two-digit quotient); nor is there any reason why the product of 12345679 by 27 should be 333333333, *except* the absolutely adequate reason that such is the inherent nature of the case. But in the realm of probabilities these exceptions help to prove the rule.

the sevens is one in four.[61] Edgeworth found that there was one chance in ten that the first 1,800 digits of the expansion of the fraction 1/1861 were following the law of chance.[62] Both he and Venn are convinced, however, that the rule of the coin is applicable to such series of digits, and indeed such is the presumption though the generality is here less well established than for games of chance.[63]

3. In 1904, long after the publication of his first papers on skew variation in homogeneous material, Pearson made the following statement:

> [Suppose] we assume a certain distribution of frequency for the character in human populations. This distribution of frequency is given by the Gauss-Laplacian normal curve of deviations from the mean. . . . Now the problem before us is the following one:—Is this assumption legitimate? It certainly is not true for organs and characters in *all* types of life. But it really does describe in a most remarkable manner the distribution of most characters in mankind . . . I should be the last to assert that no human characters can be found that do not diverge sensibly from this Gaussian distribution. But I believe they are few, and that for practical purposes we may with nearly absolute safety assume it as a first approximation to the actual state of affairs.[64]

Everything hinges here on the word "approximation." The case is very different from that of the coin or of the incommensurable number. We do not know anything about the units with which we are working except that they are the units with which we are working. When we change from the penny to the dollar we keep the same scale, one of proportions of heads in a two-fold universe; when we change from π to $\sqrt{2}$ we also keep the same scale, one of proportions of digits in a ten-fold universe, but when we change from stature to the forearm we have no guarantee that the inch means the same thing biologically. And we must use a biological unit, if we are to bind our class together by a biological concept, if we are to predicate the normal law of all *human* characters. That the form of distribution is a function of the unit is unescapable. What we need to know, and what we do not know, is whether the differences that would occur with biologically reasonable

[61] Venn, *op. cit.*, 111-118, 247f.

[62] Edgeworth, *Cam. Philos. Trans.* 20, 1905, 128-130.

[63] See also S. Newcomb, "Note on the frequency of use of the different digits in natural numbers," *Amer. J. Math.* 4, 1881, 39f.

[64] *Biometrika* 3, 1904, 142f. Many curves are given which *look* like good fits. The measures of goodness of fit obtained by Pearson and A. Lee, *ibid.* 2, 1903, 357-462, show great variation. There are equal odds that the statures of "mothers" follow the law, and only a few chances in a thousand that the forearms of "fathers," "sons," and "daughters" do. Pearson seems here, as we should like him to do elsewhere, to take his own criterion with a grain of salt.

variation of the biological unit from the physical unit are small enough to be included within the "approximation." A certain amount of alteration of the scale of measurement still leaves a sensibly normal curve. Have we any cogent grounds for assuming "with nearly absolute safety" that "the actual state of affairs" is not enough differently conditioned from those that we know about as to render it widely divergent from the normal law? This is a reasonable question, and within approximate limits it may be answerable. Certainly we may not generalize with Pearson on any other basis, and certainly Pearson neglects to provide this basis.[65]

4 On such an insecure structure does Pearson attempt to build further. If the physical is normal, why not the mental?

> I put the problem to myself as follows:—Assume the fundamental laws of distribution which we know to hold for the physical characters in man, and see whither they lead us when applied to the psychical characteristics. They must: (a) Give us totally discordant results. If so, we shall conclude that these laws have no application to the mental and moral attributes. Or, (b) Give us accordant results. If so, we may go a stage further and ask how these results compare with those for the inheritance of physical characters.[66]

(The paper is on inheritance.) The assumption of the normal law gives results for the mental characteristics that look like the results for physical characters. Hence we may go ahead with our correlations in each case to indicate the degree of inheritance. The assumption accomplishes little beyond providing the scale for the printed graphs of intelligence; but that was in 1904.

In 1914 we find Pearson still arguing for a normal distribution of intelligence, and the occasions for his argument are certain empirical results which did not show a good agreement with the "Gaussian" law.[67] He is presenting the results of the application of the Jaederholm form of the Binet scale to school children of Stockholm. The samples are very small (261 normal and 301 defective children). There is one chance in 60 that the normal children give but a random deviation

[65] He is, in fact, not clear on the part played by the unit of measurement. He is concerned much over the normal law and but little over the arbitrary scale of measurement that determines in advance the distribution.

[66] *Biometrika* 3, 1904, 147. We have seen that Pearson also assumes the "Gaussian" curve for mental ability in *ibid.* 5, 1906, 105ff.

[67] Pearson and G. A. Jaederholm, *Mendelism and the problem of mental defect, II: On the continuity of mental defect*, 1914; Pearson, *Mendelism and the problem of mental defect, III: On the graduated character of mental defect, etc.*, 1914. These papers are viii and ix in *Questions of the day and fray*. On adjustment to the normal curve see II, 28-36, 46f.

from a properly "Gaussian" distribution; there is one chance in 20 that the normal children plus an ideal fraction of the defective children are properly distributed in the "Gaussian" manner. Eight-year old children taken alone, however, do show an even chance for the applicability of the "Gaussian" law. This is slim evidence, but it appears that it is not yet time to declare the Gaussian image a false god:

> There is absolutely no reason why the Gaussian curve should be dogmatically asserted to apply to frequency-distributions of intelligence . . . Still there are *a priori* suggestions that it should be tried. In the first place it does describe with a great degree of accuracy most physical measurements in man, and secondly, the Biometric school has found that it gives good results for many measures of intelligence. It is the view of the psychological joint-author of this paper that its comparative failure as applied to the present data lies rather in faults of the tests applied or in the method of applying them than in the non-Gaussian character of intelligence when adequately measured.[68]

This quotation is as near a confession of what the Biometric School is trying to do as we are likely to get. It is frankly seeking always to see Gauss in nature. Its excuse in the case of mental phenomena for a prejudice in favor of this one of an infinity of possible distributions is that the Gaussian curve "does describe most physical measurements in man," and that "it gives good results for many measures of intelligence." The physical agreements, however, are approximate, as we have seen, and the facts with respect to intelligence are questioned.[69] The real difficulty comes, however, in the grounds for analogy. The implication is that we may reason from the physical to the mental because, in this case, both are "human," and that we may reason from one test to another because both are tests "of intelligence." Now we have no distributions of 'humanness,' but only of measurements of human characters; and we have no distributions of intelligence, but only of particular measurements of intelligence.[70] Moreover, in every case the measure is arbitrarily chosen; and it is not even strict-

[68] *Opp. citt., Mendelism, etc., II*, 46.

[69] Miner, *op. cit.*, 267-279, reviews the evidence for a normal distribution of tested intelligence, and concludes: "In spite of these arguments and the evidence of asymmetry of measurements at least of some periods of life it is to be noted that current opinion is probably contrary to this hyposthsis [hypothesis, the normal law], although, as I believe, because it has been mainly concerned with those who are not of extreme ability:" p. 275.

[70] *Cf.* here Miner's distinction between "physical units" and "more equivalent biological units," *op. cit.*, 255. We have noted that Miner does not take advantage of the full force of the distinction; the physical unit may be irrelevant to the problem in hand.

ly a measure, in the sense of being the sum of equal units, unless we make it so on *a priori* grounds. In an intelligence-test, for example, we have no notion whether the difference between 99 and 100 points is the same difference in intelligence as the difference between 9 and 10 points, unless we are ready to define intelligence arbitrarily as the ability to achieve points in that particular test.[71] Now if we define intelligence with respect to one test, we can not assert *a priori* that any other test is a measure of intelligence, and should therefore show the same distribution. *A posteriori* we may show by correlation that a second test measures an ability similar to intelligence as defined by the first test, but in such a process we demonstrate empirically the similarity of distribution. There is no valid reason for expecting normality in one intelligence-test because it has been found in another; and the same reasoning applies to the argument from the physical to the mental.[72]

We are very far, then from a general conclusion that intelligence, a mental capacity comprehensible apart from the particular instrument by which it is tested, is normally distributed. Still farther are we from stating that mental abilities, whatever their nature, follow the normal law.

5. We have to investigate one more class of phenomena, the psychophysical judgments. In this case no claim for generality has been presented, but the trend of the facts bespeaks attention. Urban found that the phi-function of gamma is a good hypothesis for the psychometric function in lifted

[71] No one who has ever "made up" an intelligence test ought to have any difficulty in appreciating the arbitrariness of the calibration. The tasks of the test are in the first place sheer casual invention. They are put into a scale and tried out. Alterations are made to "improve" the frequencies of success in different component parts, but no one can work accurately from the frequencies to the scale without presupposing the form of distribution. There is never any attention paid to making equal increments of intelligence measured by equal increments of the scale, for the excellent reason that no one has any such definite quantitative conception of intelligence. To call a year in mental age a unit is a fallacy of this sort. See Pearson, *Mendelism, etc.*, II, 36*ff.*; Miner, *op. cit.*, 26off. There is no evidence (nor are the physical growth curves in any way relevant) that annual increments *of intelligence* are equal in any other sense except in that they are annual. Of course a year is a year, if "annual-ness" is what we are after.

[72] Pearson has appointed himself "watch-dog" of "logic in scientific procedure", *Mendelism, etc.*, III, 3f.; but he seems to need to make friends with one sophistical wolf ("an appearance of knowledge where we are as yet in a state of ignorance") in order to attack another.

weights and in certain acoumetric experiments.[73] Now the phi-function of gamma is simply the integral form of the normal curve. To say that the phi-function of gamma is the psychometric function is equivalent to saying that the dispositional variations of the psychophysical organism, when measured in a scale of units proportional to the scale in which the stimulus is measured, follow the normal law. If the lifted weights are measured in grams and the psychometric function approximates the phi-function of gamma, then the organism varies in the amount of its disposition for judging "heavier" or "lighter" according to normal law, provided always that the amount of this disposition is measured in grams. This relationship is scarcely obvious and the reader may need to read elsewhere,[74] before he can accept it. The essential thing to understand is that the applicability of the phi-function of gamma means that the organism is varying somehow in accordance with the normal law, but that the normality of its variation is as much a function of the unit in which the variation is measured as it is in any of the other cases which we have considered.

Now can we generalize? Can we ever say that all psychophysical judgments tend to obey the normal law? Urban makes no such attempt; rather is he against theorising and for description. "The nature of the dependence," he writes, "is not known and cannot possibly be deduced by any considerations *a priori*;"[75] and he refers us for the function to "the results of observation." This, to the scientist, is a refreshing return to facts from the realm of logic and mathematics; but is not final. Something can be said for generalization.

Unfortunately for ease of exposition, there are two kinds of psychophysics. In the one kind stimuli are presented and the observer reports upon the sense-impressions. In the case of the stimulus-limen, he simply reports sensation or its absence. He is making a brief introspection. In the case of the differential limen he makes a judgment upon the relative degrees of the two sensations, but the judgment is actually little more than a comparative description; the ob-

[73] On lifted weights: Urban, *op. cit.*, 257ff.; *Arch. f. d. ges. Psychol.* 16, 1909, 168-227, espec. 224f. On acoumetry, H. Keller's experiments, *Psychol. Stud.* 3, 1907, 49-89; and Urban's discussion of them, *Arch. f. d. ges. Psychol.* 18, 1910, 400-410. Keller used Wundt's fall phonometer, and the all-important unit of stimulus, which is only just mentioned, is the cm. The psychometric functions of the lifted weights are for grams.

[74] Boring, *Amer. J. Psychol.* 28, 1917, 465ff.

[75] *Psychol. Rev.* 17, 247.

server may be said to be introspecting here too.[76] In the other kind of psychophysics the observer's attention is directed upon the stimulus and not upon his own mental processes. In no sense does he introspect. Always he is making judgments of the stimulus. In introspective psychology this attitude is called the "stimulus error," but in psychophysics it may be in place. Urban's experiments are of this kind,[77] as has been much other psychophysical work. There is reason to believe, however, that psychology will profit more in the long run by work performed under the first attitude described.

In the case of the "introspective" psychophysics we are not to expect a generalization, because everything depends on the unit. Were the psychometric function to approximate the phi-function of gamma ten times, there would be no reason to expect it the eleventh. Suppose the eleventh case were the differential limen for auditory intensity with the Fechner sound pendulum as the instrument. Will the normal law hold? Perhaps when we use as the unit $\sin^2(\Theta/2)$. If so, it will not hold if we use Θ as the unit. If it holds for neither, it may hold for some other function of Θ. The reports upon sensations depend upon Θ and the momentary disposition of the organism, but in no way upon the scale of units chosen. It is mere luck if the normal law applies. But whatever luck we get is good luck, for the psychometric function gives us the law of the dependence of dispositional variation upon the scale of measurement which we have chosen. A little knowledge of sensory mechanisms is worth the loss of a generality.

When the psychophysical judgments are directed upon the

[76] *Cf.* the nature of the constant psychophysical attitude set up in S. S. George's experiment, *Amer. J. Psychol.* 28, 1917, 1-37.

[77] In the discussion of Ueber einige Begriffe und Aufgaben der Psychophysik, *Arch. f. d. ges. Psychol.* 30, 1913, 113-152 (*cf.* also, *e. g.*, *Amer. J. Psychol.* 24, 1913, 274), Urban would appear to maintain that psychophysics deals with describable mental processes; but the implications in the accounts of his experiments, the lack of a reproduction of definite instructions given the observers, and the failure to discuss the introspective competence of the observers, all indicate that the observers were, or at least may have been, making judgments of stimulus rather than of mental process. See *The Application of Statistical Methods to the Problems of Psychophysics*, 1908, 5, 14; *Arch. f. d. ges. Psychol.* 15, 1909, 264f. In *Psychol. Rev.* 17, 229, he speaks of "the judgments of a subject who compares two stimuli." And indeed in the systematic schema of *Arch.* 30 (*op. cit.*) he takes account (1) of mental contents and processes, (2) of physiological conditions and processes, and (3) of physical conditions and processes, but leaves no place for meanings, awarenesses, the stimulus in relation to the subject. It is doubtful if Urban sought at all to distinguish between *Beschreibung* and *Kundgabe* in the psychical aspect of the psychophysical system.

stimulus it may be that we can generalize, for now the scale and the judgment are related. With the sound pendulum we might be asked to judge the height of the fall, which is measured by $\sin^2(\Theta/2)$, or we might be asked to judge the angle. In the two cases we would be judging entirely different things, and it is quite possible that the normal law might apply in each. If we found that it applied in a large number of cases, then we might generalize by saying that errors in the judgment of a stimulus, that is to say, errors of observation, are distributed normally. This conclusion, indeed, is Urban's: "We thus obtain the remarkable result that the foundations of the theory of the errors of observation are found in the theory of psychophysical measurement."[78] In other words we would demonstrate psychophysically Gauss' original assumption that positive and negative errors are equally likely.

If there is any general answer to the question: When can we generalize?— it is this. We may expect the normal law to hold within an entire class of phenomena, when it holds for a number of cases within the class, provided always that all variations of members of the class are capable of being expressed as variations of some common denominator. In other words, we must be able to see the same thing varying in different situations. With the penny and the dollar, the law holds for the turning up of heads. The things that make the penny different from the dollar, we know to be irrelevant. In lifted weights and acoumetry we may know, by a control of attitude, that we are studying, not kinaesthesis and auditory sensation, but errors of observation. If Urban did know this, then he was supplied with the only possible ground for expecting the one result on the basis of the other.

4. The Logical Possibilities (Conclusion)

At last we are ready to take account of stock! There are four logical possibilities, but the circumstances of the case are such that it appears unlikely that the actual mental measurement can make much use of more than one. Let us see.

1. In the first place, there is the possibility of determining *a priori* the form of distribution. This course, applied to the normal law, is popular, but it is usually without sanction. In quantitative psychology we should resist it. We may not assume the normal law in an interpretative study except upon

[78] *Psychol. Rev.* 17, 243; but Urban is being betrayed into generalizing. His statement is based only on the lifted weights and perhaps the acoumetric experiments. The validity of generalization under this attitude is hardly to be demonstrated so easily.

valid grounds. We have seen that the law of insufficient reason furnishes no such grounds. Knowledge simply does not come out of ignorance. The only scientific grounds that are presumptive of the normal law are an intimate knowledge of the constitution of the particular case and of the function that frequencies of occurrence are of that constitution. Such a function comes in science from experience, and, whenever it is thoroughly understood, from experiment. On such cogent grounds we may conclude *a priori* to the normal law or some other form of distribution, and we are following such a logical process in the few cases where we find it legitimate to generalize about the form of distribution for a given class of phenomena.

Two corollaries follow.

(a) If we knew that the normal law had to hold in a particular case, then we could experiment with an arbitrarily chosen unit and determine, by working from the law, the function that the "true" psychological unit, which is following the law, is of the arbitrary unit of the experiment. Any such hope, however, is bound to be illusory, for a knowledge of the constitution of the case involves *a priori* the knowledge of this unit. We may say that, if the dollar is like the penny, we shall get 50% heads; but we can not reverse the argument and say that, because we get 50% heads, the dollar is like the penny.[79]

(b) There is at present very little prospect in the field of mental measurement that our knowledge of the psychological constitution of a mental function or process will be sufficient to enable us to begin work upon a problem with the assurance that the resulting distributions must be normal. There is a bare hint of an exception, that awaits verification, in the psychometric functions of errors of observation. But the evidence indicates that instances, where there are cogent reasons for assuming the normal law in advance of empirical determination, will be extremely rare for a long time to come.

2. We may begin in mental measurement with the psychological unit, for if we can not determine the unit from the distribution, we may, nevertheless, determine the distribution from the unit. Such, in fact, is the necessary scientific order.

[79] *l. c.*, in physical constitution. We are not denying inverse probability, but insisting merely that a statement of physical constitution is more complex than a statement of frequency and that the same frequency may be variously conditioned. If we know only that we get 50% heads, we can not reason that the underlying physical constitution is that of the dollar or the penny. For all we can tell we may be dealing with an asymmetrical loaded die, with one head and five tails, that belongs with the penny only in the matter of frequency of heads and not in general physical constitution.

The great difficulty is, as we have just pointed out, to find anything that we may properly call a psychological unit. The sense-distance is such a unit. It is a unit of measurement that is mental *per se*.[80] Most of the sensory work in psychology, however, has had to do with the determination of limens and has not made use of the mental unit at hand. The extensive psychophysics of memory has not achieved a psychological unit, nor have the other departments of the psychology of process. In the psychology of capacity or function the case is worse. For example, with intelligence, which is the mental capacity most often measured, we have seen that, not only is there no attempt to make equal increments on an intelligence scale correspond to equal increments of intelligence, but that the concept of intelligence is so vague that any such accurate quantitative relationship is in practice almost meaningless. We are not, however, at our rope's end.

3. If a psychological unit is not to be achieved we may use a "physical" unit, that is to say, some arbitrarily chosen aspect of an arbitrary scale of measurement. Such units are the year (mental age), the second (mental tests where time is the measure), the item or task (where the number of points is the number of unit tasks completed, *e. g.*, the U. S. Army intelligence tests), the gram (lifted weights), the syllable (memory experiments), and so on. Every such unit is arbitrary. There is no evidence that equal increments of its scale correspond to equal increments of the psychological entity measured. When we define intelligence, in order to gain definiteness of conception, as ability in some particular test, we are simply substituting a "physical" unit for the ideal, but unachievable, psychological unit. Now the application of a unit that is not psychological to a quantity that is psychological does not yield a measure of the quantity. It will place the quantity in a given position upon the arbitrary scale, and will determine the rank-orders of a number of quantities so placed, but the assumption that rank-orders tell the amount of a given quantity, or, if the zero be unknown, of a given increment, is, of course, unwarranted.

There is one case, of frequent occurrence in the behaviorism

[80] *Cf.* E. B. Titchener, *Experimental psychology, Quantitative student's manual*, 1905, xix-xxxvii. The thing is so simple when it is seen! In physical science the nature of the unit is given in the presuppositions of the science, and in psychology it is given mentally in the sense-distance. It is the indefiniteness of the mental function that makes trouble and gives rise to the attempt to create a unit in retrospect by an appeal to the hypothesis of the normal law. Since this paper deals with the appeal to the normal law, we may, however, pay homage to the sense-distance and proceed.

of mental tests, where the "physical" and psychological units seem to become identical. Suppose we are studying the learning of typewriting and express our results in the number of words written per minute. Do not words-per-minute truly measure ability in typewriting? Here we are involved in a question of point of view. Words-per-minute measure the product of typewriting. They may perhaps be said to measure ability in typewriting from the point of view of the employer of a stenographic force; they are the ability of his office force over against a given job. A behavioristic psychology that identified behavior with mere product, without any reference at all to the conditions of the product, might take the point of view of the employer; but such a psychology would achieve only a physics, or more likely a common sense, of typewriting. Any definition of behavior as *response to a situation* [81] brings in the behaving organism and changes the ground. We can not say that words-per-minute truly measure the behavior of the psychophysical organism or its response to the situation. Does an increment of ten words per minute mean the same change in ability or in behavior or in response when added to an ability of 50 words per minute as when added to an ability of 100 words per minute? To the employer, yes; but not to the psychologist, or, presumably, to the organism. It is indeed an heroic measure, when we can not make the unit psychological, to attempt to a-psychologize psychology.

4. We are left then with the rank-orders of our psychological quantities, given by reference to a fixed but arbitrary extra-psychological scale; and it is with these rank-orders that we must deal. We are not yet ready for much psychological measurement in the strict sense. It is true that psychophysics may yield a unit and also true that ordinarily it does not. The sense-distance has had only a limited use, and we do not yet know that the stimulus-distance (for it is upon stimulus-distances that the psychometric functions of errors of observation are founded) constitutes a true unit in a behavioristic psychophysics. The case, however, is not so bad as it seems at first. There is nothing new in the contention that mental measurement is impossible,[82] whereas now we do gain the assurance that rank-orders at least are validly demonstrable. And there

[81] *Cf.* the point of view of behavior presented by E. B. Holt, *The Freudian wish*, 1916, espec. 153ff. Behavior is *what* the individual is *doing*, not that he is moving. We can perhaps measure movement, but we have yet to conceive the unit for "doings."

[82] Indeed, in confining ourselves to a consideration of the part played by the normal law, we are leaving a famous controversy untouched. *Cf.* O. Klemm, *A history of psychology*, trans. 1914, 150-155, 232-267.

is a great deal that can be done with rank-orders. We can deal with frequencies, medians, and quartiles. For example, it is considerable to know of two groups that the lower quartile of the ranks in one overlaps and is practically coincident with the upper quartile of the other. What we must remember, however, is that we are dealing with the statistics of medians, quartiles, contingencies, and correlation ratios; not with the statistics of averages, standard deviations, coefficients of correlation, and linear regressions. All those statistical constants, that imply a scale of equivalent units, violate in use the conditions of the case and lead to a precision of result that is an artifact. The serial constants, that do not presuppose a unit, yield less intricate resultants, but they present a rougher picture that represents truly the rough material which they describe.[83]

The initial error in the application of the theory of probabilities was the assumption of the law of insufficient reason. It was wrongly supposed that knowledge could somehow be wrought out of ignorance. This very error, however, has never been routed. It has gone on, multiplying mischief. The substitute for insufficient reason is cogent reason. The more we know of the intimate nature of the entity with which we are dealing the more accurate and complete can our descriptions become. But, if in psychology we must deal—and it seems we must—with abilities, capacities, dispositions and tendencies, the nature of which we can not accurately define, then it is senseless to seek in the logical process of mathematical elaboration a psychologically significant precision that was not present in the psychological setting of the problem. Just as ignorance will not breed knowledge, so inaccuracy of definition will never yield precision of result.

[83] *Cf.* C. J. West, *Introduction to mathematical statistics*, 1918, 94.

FOVEAL ADAPTATION TO COLOR[1]

By HUBERT SHEPPARD

CONTENTS

		PAGE
I.	Introduction: Historical	34
II.	Experimental: Plan of the Experiment	36
III.	Part I.: General Adaptation in Direct Sunlight	37
	Foveal Adaptation to Surface Colors (Preliminary Series)	43
IV.	Part II.: Foveal Adaptation to Surface Colors Under Controlled Conditions	44
	Chroma as a Determinant of Adaptation	49
V.	Part III.: Foveal Adaptation to Spectral Colors	50
	Equation for Brightness	52
	Chroma as a Determinant of Adaptation	52
	Adaptation at High Intensities	55
Conclusions		58

INTRODUCTION

The primary aim of this study is to describe the course of foveal adaptation to color. We have also obtained records of the times of adaptation, and have attempted to show their variation with variations of intensity and chroma; but our main purpose throughout the experiment has been to secure introspective reports of the attributive changes which occur while the fovea or macula lutea is under stimulation by a color, from the moment of exhibition until the color has disappeared or until there is no further alteration of hue or tint or chroma. The peripheral retina is discussed only in so far as its discussion bears upon adaptation at the fovea.

Wherever possible, all earlier experiments which seemed to relate, directly or indirectly, to our problem were repeated, and their results tested, before our own technique was finally decided upon. Very little work has been done, however, that bears directly upon foveal adaptation considered as a mode of experience. Adaptation in general depends (1) upon wave-length, wave-form, and wave-amplitude of light, and (2) upon the time during which the light-waves affect the eye. Hence the problem may be approached from the side of light-dark adaptation, of color adaptation, or of negative after-image. Previous investigators have studied the first and third

[1] From the Psychological Laboratory of Cornell University.

of these topics rather than the experienced course of color adaptation.

The term *adaptation*, as applied to visual experiences, was first used by Aubert, to denote "the accommodation of the eye to light-intensities."[2] Nowhere in his writings does Aubert connect this term with color-changes, for which he uses the term 'fatigue.' In his paper on fatigue and retinal after-image,[3] as elsewhere, he argues that there is a close analogy of function between the fovea and the peripheral retina. Colored objects, he says, appear colorless in both direct and indirect vision, if the stimulus is sufficiently long continued. He is, therefore, familiar with complete adaptation, but includes it under the term 'fatigue.'

Exner, working under the direction of Helmholtz, made the first systematic study of adaptation (fatigue) to color-stimulation.[4] In an experiment in which he describes the course of the after-image, he finds that three parts of the spectrum are much less altered in appearance by 'fatigue' than other parts, namely, *R* from the end of the spectrum to a point between the lines C and D, *G* between E and b, and *B* near G. He notes also that, if the after-image of a color is projected upon another color of the same hue, this second color appears much less saturated.[5]

Von Kries investigated the problem of adaptation from both the physical and the psychological points of view. He found that "with long fixation, a color loses in brightness as well as in saturation."[6] suffer no change in hue, while the other colors change in the direction of *Y* or *B* from *G*.

Hess made a study of the tone-changes of spectral colors when the retina was fatigued with homogeneous light.[7] He observed that all colors possess either a *Y* or a *B* valence. That is, all colors, from near a pure *R* to a pure *Y*, or near a pure *G* to a pure *Y*, will change under adaptation towards *Y*; and all colors from near a pure *R* to a pure *B*, or near a pure *G* to a pure *B*, will change towards *B*. Further, during the process of adaptation the stimulus-color always becomes less and less saturated, until it disappears; the complement of the stimulus-color always remains unchanged in hue; and the appearance of a spectral color after adaptation corresponds in general to a mixture of that color with its complement.

Burch was able to produce temporary color-blindness by means of general adaptation at any intensity.[8] In an article entitled "On the Production of Artificial Colour-blindness to Successive Contrast" he describes the process of adaptation as complete for *R*, *G*, *B* and *V*. In another experiment he was able to get complete adaptation in 3 min. when the intensity of the stimulating light was bright moonlight. His method for very high intensities was as follows: The eye of an

[2] H. Aubert, *Physiol. d. Netzhaut*, 1865, 39 ff.

[3] Moleschott's *Untersuch.*, IV, 1858, 220 ff.

[4] J. Aitken, *Roy. Scot. Soc. of Arts, Proc.* 1871-2, and E. Hunt, *Colour Vision*, Glasgow, 1892, made similar studies of adaptation. A detailed account of their method and procedure is given later by Burch.

[5] S. Exner, 'Ueber einige neue subjective Gesichtserscheinungen,' *Arch. f. Physiol.*, I, 1868, 375-394.

[6] Nagel's *Handb. d. Physiol. d. Mensch.*, III, 1905, 213-220.

Further, *Y* of a wave-length of 560 $\mu\mu$, *G* of 500 $\mu\mu$, and *B* of 460 $\mu\mu$

[7] C. Hess, *Arch. f. Ophth.*, 1890, XXXVI, Abtl. 1. 1 ff.

[8] G. J. Burch, *Phil. Trans.*, 191 B, 1899, 1-135. *Roy. Soc. Proc.*, 66, 208 ff, 216-219.

observer was exposed to bright sunlight, which had been focussed by a burning-glass through a colored screen. After the eye had been subjected to this intense light until all sensation of the particular color was lost, the observer proved to be temporarily color-blind to the color used in stimulation. Further, four parts of the spectrum, *R* from A to B, *G* in the neighborhood of E, *B* about half-way between F and G, and *V* at and beyond H, showed marked fatigue-effects, differing in degree but not in kind. All direct sensation of the stimulus-color was temporarily lost: *R* for 10 min., *G* for a still longer period, while the recovery from *V*-blindness might require a couple of hours. Whenever the eye is completely fatigued to any color, that color is absent from the spectrum, whatever the fatiguing intensity may have been. The observer is, in fact, color-blind for the particular color. After fatiguing the eye with three of the four colors, Burch found that only one color remained.[9]

Troland has unpublished data from which he is able to determine a theoretical course for the process of adaptation in all the adapting sense departments. Retinal adaptation follows the same general course as that of the other adapting organs.[10] The rate of the process is greater in the beginning, with a decrease rapid at first, then more gradual, until it finally reaches a level at which it is constant.[11] Troland finds also "that, at least for stimuli of fairly high intensity, adaptation does not proceed to a point at which the resultant sensation reduces to a neutral gray."[12]

In sum, then, Exner, von Kries, and Hess come to almost exact agreement with regard to the gross results of adaptation to color. (1) The 'primary' colors, *Y* 550-574 $\mu\mu$, *G* 495-520 $\mu\mu$, *B* 435-471 $\mu\mu$, do not change in hue under the process of adaptation. Hess finds a psychological *R* outside of the solar spectrum which also does not change. All other colors change either towards *Y* or towards *B*. (2) A color, after adaptation, corresponds in general to a mixture of this color with its complement.

As regards adaptation to color at high intensity, there is difference of opinion. Burch gets complete general adaptation with any intensity which produces temporary color-blindness to the stimulus-color. Troland, on the other hand, finds that color-adaptation at high intensity does not reduce the color to a neutral gray.

Experimental: Plan of the Experiment

The present experimental work upon foveal adaptation to color was begun in October, 1918, and ended in September, 1919. The observers were Dr. C. W. Bock (B), Mr. H. Sheppard (Sh) and Mr. M. J. Zigler (Z), instructors in psychology, and Mr. H. Liddell (L), Miss A. K Sullivan (S), and Miss M. Finck (F), graduate students in the department. L, S and Z were given several weeks' training before any data were taken for our records; F was relatively unpractised.

[9] A. W. Porter and F W. Edridge-Green, *Proc. Roy. Soc.*, 85 B, 1912, 435 ff, find somewhat different results for *B* and *G;* but Burch, *Proc. Roy. Soc.*, 86 B, 1912-13, 117-18, argues that their work was not carefully done, and that their results are not strictly reliable.

[10] L. T. Troland, *Am. J. Psych.*, *25*, 521 ff.

[11] *Ibid.*, 508.

[12] *Ibid.*, 520.

Our object, as has been stated above, was to describe the course of foveal adaptation to color under as many different conditions as possible. We do not attempt a theory, nor do we draw any theoretical conclusion whatever.[13] We aim only at a qualitative study of the experienced course of adaptation, with secondary reference to the times of adaptation for the different colors used.

The experiment as a whole comprises three parts. In Part I. we examine general adaptation to a surface color in direct sunlight. Here the experienced course of adaptation is described in its various stages. In this preliminary experiment we also take account of foveal adaptation in a general and tentative way. In Part II. we work with surface-colors, and the process of adaptation takes place under certain controlled conditions. These conditions greatly reduced eye movement, and kept the illumination constant. Here we describe the experienced course of foveal adaptation to 9 different colors, *R, O, Y, YG, G, BG, B, V,* and *C*. In addition we consider the effect of chroma upon adaptation. In Part III. pure spectral colors are used with various intensities of illumination. Here we describe the experienced course of foveal adaptation, and note in what respect this process differs from the course of adaptation when a surface-color is used. Difference of chroma is also considered. Two colors, differing in hue, are matched for chroma, and the process and time of adaptation are recorded in each case. We also give the results of some experiments made with high intensities of stimulus.

Part I.: Preliminary Experiment

General Adaptation in Direct Sunlight

We are here concerned to determine the course of general adaptation when a surface-color, illuminated by direct sunlight, stimulates the entire retina. In former experiments peripheral adaptation to color has been found to be more rapid than foveal.[14] Our main problem throughout the preliminary experiment was the study of the course of adaptation, by introspective observation, as the process travelled over the retina from periphery to fovea. If there is a rapid fading-out of a color under peripheral stimulation, as is indicated in the literature, it ought to be possible to observe this change in its progress towards the central retina.

[13] Papers on 'An Experimental Treatment and Discussion of the Theory of Adaptation and Color Vision' as well as 'The Early Development of Color Vision' are now under consideration and will follow shortly.

[14] J. W. Baird, *Color Sensitivity of the Peripheral Retina*, 1905, 53-74.

Procedure.—A drawing-board of 28 x 36 in. was fastened in a window on the west side of the optics room. The board served as a table upon which to place the stimulus-color. The following papers were selected for stimuli in the preliminary tests:

Color	Brightness Value		Match to a Gray	Brightness Value of Match	
	Black	White		Black	White
R	288°	72°	211°	88°	272°
BG	206	154	149		
Y	144	216	170	154	106
B	263	97	190		
GY	162	198	125	112	248
V	285	75	235		
C	259	101	208		
G	250	110	132	97	263
B					
O	216	144	92		
G			120	112	248
B			148		

A sheet of colored paper, 19 x 24 in., was spread upon the table. In the center of the sheet was a very small light dot for a fixation-point. *O* sat facing the stimulus with one elbow resting upon the table and the chin in the palm of the hand. This position brought the eye within about 25 cm. of the fixation point, and was found to be convenient for fixation in monocular vision. All the experimental work for this part of the experiment was done during the months of July and August, from 2 to 4:30 o'clock in the afternoon. We were careful to select days when the sky was free from clouds or haze.[15]

The instruction to *O* was to report all change in color sensitivity, from the time the eye first fixated the stimulus until the color either faded out to a gray, showing no trace of color, or reached a stage where no further change in hue, tint,

[15] It is very difficult to get complete adaptation to *R, BG, B, C* and *V* in a reasonable length of time unless the sun is shining brightly. The sky must be free from clouds or haze, or the times of adaptation will vary considerably.

or chroma was perceptible. *O* was asked to give a running report of all sensory change, and in as much detail as possible.

Results.—The results are given in summary for the different observers and stimuli.

Observer B. Stimulus *Y.*— A gray film soon appeared in front of the stimulus-color. This film came in from the periphery. There was a slow change in chroma until the color faded out to a white or very light gray. A small area surrounding the fixation-point persisted longer than the other parts of the field. The change was slow at first, then more rapid towards the last.

Stimulus *BG.*— The outer edge began to pale rapidly at first. This paling extended inward towards the center. The center remained richer in chroma than the outer edge for some time. Finally the surface evened up into a gray field.

Stimulus *B.*—There was no change in hue. The haze appeared over the surface from the outside, although very slowly as compared with some of the other colors. The color persisted for a considerable time around the fixation point. In the process of fading out there were many bluish spots scattered about among white filmy spots. These spots would even up into a white or light gray surface.

Stimulus *R.*— While the color was fading out from the periphery, certain portions did not appear uniformly colored; little filmy red dots were scattered about over the surface. Finally the red spots paled out to gray, and then evened up over the whole surface. The color around the fixation-point remained for some time as a dirty brown color. The final gray surface was very dark.

Stimulus *V.*— There was a rough cloudy surface over the color for a considerable time. The color began to pale out from the outer edge, and the change traveled towards the center. The roughness always smoothed out into a gray film of a thick texture. It is very difficult to tell just when adaptation is complete.

Stimulus *O.*— It faded into a beautiful light yellow, beginning on the outer edge and gradually decreasing towards the fixation-point. From the *Y* it went into a gray which was somewhat darker than the *O*. The steps were perceptible though gradual. All changes were from the periphery towards the center.

Stimulus *C.*— Almost immediately the surface began to grow white in the outer portion of the field. This white gradually approached the center. The final stage was a lustrous silvery gray.

Stimulus *G.*— There was a slow change in hue from *G* through *Y*. The *Y* finally changed into a very light gray, very much lighter than the stimulus-color.

Stimulus *GY.*— The green faded out of the periphery very quickly. This process always took place before the *Y* began to go. The *Y* then changed into a very light gray. The surface of the stimulus-field at times had a rough, uneven appearance. This gradually changed into an even surface without any color.

Observer Sh. Stimulus *Y.*— Almost immediately the color began to grow lighter and the chroma poorer. There was no change in hue. As soon as the color began to fade a filmy surface appeared over the surface of the color. The fading out process always came from without towards the fixation-point. The periphery became white some time before the fixation-point. The final stage was an almost white surface.

Stimulus *BG.*— There was a gradual, though rapid decrease in saturation from the beginning. Also there appeared a very rough, uneven surface about the fixation-point, which was much more satu-

rated than the rest of the field. This area gradually evened up into a gray. The hue changed slightly to *B*.

Stimulus *B*.— The blue faded out very slowly from the periphery to the center. At all times the hue appeared to shine through a white filmy surface which formed over the color. This effect persisted to the end.[16] The complete stage of adaptation was a very pretty whitish gray, like an after-image of a velvet-black surface. There was no hue-change whatever that could be observed. The entire process was a slow fading away of the color into a filmy surface. Two cases of staring blindness occurred during this series of observations.

Stimulus *R*.— The red is unlike any other color in adapting out to a gray. It passes through different stages of hue-change from the first. Almost immediately it passes over into a red-orange, then to an orange of poor chroma, and finally into a dirty, dingy brown, which is very persistent. This brown finally goes into a poor gray, then to a good gray surface.

Stimulus *V*.— Violet is the most stubborn of all the pigment-colors to a complete adaptation in sunlight. There is a feeling of uncertainty as to the judgment, because the violet is almost of the same brightness as the gray surface which persists when adaptation is complete. (The two are so near alike that it is often difficult for *O* to decide when the final stage is reached.) There is a rough cloudy surface at first, which gradually smooths down with time. Two changes in hue always take place. At first there is a tendency for the red to persist; this presently adapts out, and leaves a bluish-violet surface of very poor chroma.

Stimulus *O*.— Orange adapts out very much like red from the second stage of the red, except that the orange is richer in chroma than the red at this stage. When the color reaches the brown stage, it is almost exactly like the same stage in the red, except that the orange is brighter than the red. One effect of staring-blindness was observed, which lasted for three or four seconds.

Stimulus *C*.— Almost immediately the surface began to take on a bright lustrous appearance. This effect gradually thickened, until the whole surface was of a beautiful silver lustre. The hue changed somewhat to blue, thus causing the surface to become darker.

Stimulus *G*.— For the first few seconds the color did not appear to change. Suddenly there is a change, the yellow appears and the green seems to weaken. From this point there is a rapid fading away. The yellow appears to go out first; the green, now much less saturated, fades out into a beautiful gray surface which is very smooth in texture. Two cases of staring blindness were observed.

Stimulus *GY*.— The color is stubborn at first; the green suddenly goes out; and then there is a gradual, though rapid, fading away of the yellow to a beautiful gray surface. After the green passes away, the process of adaptation is very much like that of the yellow. The final stage, the gray surface, is similar to the same stage in the yellow adaptation.

Observer Z. Stimulus *Y*.— The color began to fade in the periphery almost immediately; this caused the center to appear much lighter, and richer in chroma. The small center persisted for some time, although slowly fading all the time. Finally the whole surface of the stimulus-color became a uniform gray

[16] To get the effect of the soft white surface, when adaptation is complete, a Hering blue must be used. A paper that has a glossy surface is very difficult to observe for this length of time.

Stimulus *BG*.— There was no apparent change in hue. A thin filmy mist came in from the edge of the paper, which caused all color to disappear from the periphery. This colorless effect gradually went towards the center, until the whole color disappeared.

Stimulus *B*.— No hue-change. The light or white foggy mist approached the center from the periphery until the whole surface was a very poor *B*, almost gray. This state persisted for some seconds. Finally all color faded away, leaving only a gray field.

Stimulus *R*.— There was always a decided hue-change. It went first to a very poor yellowish *O*, then to a dirty yellow gray. This persisted for some seconds. The final stage is a peculiar gray. It appears to be a little darker than the stimulus-color.

Stimulus *V*.— There was a slight hue-change to *B*. The decrease in chroma was marked at first. However, the final state is difficult to judge, since the final gray and the *V*, very poor in chroma, are much alike.

Stimulus *O*.— There was a change in hue to *Y*, poor in chroma. This change began in the periphery, and traveled towards the center. Finally the center went out, exactly like the *Y*.

Stimulus *C*.— This is a very stubborn color. It appears to be uneven in surface during the adaptation-process. It changes from periphery to center, but not so markedly as the other colors. The rough uneven surface seems to smooth down into the dark gray film as it thickens over the surface. There is a hue-change, but it is difficult to observe in its course. It would appear to change to *B*, although this can not be observed clearly.

Stimulus *G*.— Very slow change to *Y*. This change only comes about when the color has lost nearly all chroma. The film thickens from the periphery towards the center. This gradually conceals all color. The resulting gray surface appears to be a little lighter than the stimulus-color.

Stimulus *GY*.— The periphery began to fade very quickly. For several seconds this part of the field looked like wool, while in the center of the field there was a *YG* of very good chroma. Finally the center began to fade out, rather suddenly, to a good gray surface. The hue changed to *Y*, although the final stage came when the color was very low in chroma.

Observer S. Stimulus *Y*.— The film came over the surface from the periphery in 20 seconds. There was no apparent change in hue. The final stage was a colorless paper.

Stimulus *BG*.—No change in hue. The surface began to fade out from the periphery, all the time going towards the center.

Stimulus *B*.— Same process as the *BG*. The final gray was somewhat lighter than the stimulus-color.

Stimulus *R*.— There was decided hue-change as the color began to grow less saturated by the gray film which came in from the periphery to the center. The color went through a poor *O* to a very weak muddy *Y*. This last stage persisted for some seconds.

Stimulus *V*.— This is a very stubborn color. It is difficult to decide when all the color has faded out. The film began to appear on the periphery very quickly, moving to the center. The final stage appears to be of about the same brightness as the stimulus-color. There is a hue-change to *B*, although it is very difficult to judge.

Stimulus *O*.—The film began in the periphery. For a little, this seemed to hang on the outer part of the color. Finally it moved over

the center. At this time the hue changed to a very poor *Y*. Then the process was the same as *Y* in the last stage.

Stimulus *C*.— The color made some change in hue, but it was so slow and gradual that it was difficult to judge. There was a peculiar uneven surface during the course of adaptation. The cloud appeared from the periphery, moving to the center. This gradually weakened the chroma.

Stimulus *G*.— A hue-change to yellow in the final stage. The chroma gradually grew less, the center remaining longer.

Stmulus *GY*.— Same as *G*, except that the change to *Y* was more marked.

The first part of the accompanying table shows the adaptation-times under the conditions described. The second part shows times obtained, still in direct sunlight, when a disc, composed as indicated in the table, was viewed against a large grey background. Observation in these circumstances was difficult, owing to the lack of a fixation-point.

ADAPTATION TIMES, IN SECONDS.

AVERAGE OF FIVE OBSER ATIONS

Obs.	Sh		B		Z		S		
Color	Av.	M.V.	Av.	M.V.	Av.	M.V.	Av.	M.V.	Av. of Av.
R	198.2	6.5	192.6	10.5	190.8	9.4	194.8	5.0	194.1
O	89.6	3.9	89.4	6.9	91.8	5.4	93.8	1.4	91.1
Y	45.6	3.9	39.2	3.8	43.6	6.3	55.0	1.2	45.9
YG	49.2	7.1	53.2	5.8	50.4	6.5	52.4	7.4	51.3
G	52.0	6.8	54.6	9.5	49.6	2.9	56.6	3.5	50.6
BG	126.6	5.6	109.4	11.9	115.2	9.3	128.0	4.0	119.8
B	247.6	4.7	220.2	12.9	201.8	8.6	211.4	4.7	220.0
V	200.0	8.4	201.4	4.7	201.8	1.7	234.0	10.8	209.3
C	102.0	8.4	100.0	10.0	108.0	1.6	111.0	4.8	105.2

180° OF COLOR + 180° OF BLACK AND WHITE (BRIGHTNESS-VALUE)

R	93.8	7.0		104.0	8.6	96.8	12.6	98.2
O	42.0	5.2		45.5	3.2	45.2	4.2	44.2
Y	21.0	2.0		27.0	5.4	22.2	1.8	23.4
YG	25.4	4.7		29.0	2.1	26.0	4.8	26.8
G	26.0	7.8		23.5	6.6	21.6	4.3	23.7
BG	57.0	4.8		50.3	3.4	56.0	6.4	54.4
B	118.0	13.6		122.0	2.6	123.8	7.0	121.3
V	108.0	6.4		98.6	4.8	112.8	7.1	106.5
C	53.4	7.1		44.3	2.4	52.0	6.0	49.9

FOVEAL ADAPTATION AT MEDIUM INTENSITY

PRELIMINARY SERIES

We next sought to determine the experienced course of adaptation to color, at medium intensity, when only the fovea was stimulated by the color.

For stimuli, a circular disc, 100 cm. in diameter, was cut from each one of the 9 colors shown in the preliminary experiment. Every disc was mounted upon a gray background of 24 x 30 in. The background was free from any trace of color, and had a brightness-value of 140° white and 220° black. A frame on a table held the stimulus-card upright. The stimulus was placed at a distance of 225 cm. from the eye of the observer; the image of the stimulus-color fell within the macula.

The experimental work was done in a large light optics-room. Light window-shades were drawn over the windows, so that the room was mildly illuminated with a soft white light. The instructions to *O* were the same as in the preliminary experiment.

The quantitative results (times in sec.) are set forth in the following table. The averages are of 5 observations.

Obs.	Sh.		S.	
Color	Av.	M.V.	Av.	M.V.
R	137	7.5	178	8.1
O	140	6.5	140	8.4
Y	100	4.5	105	4.2
YG	88	4.5	95	7.0
G	80	5.5	110	6.2
BG	100	3.2	98	3.1
B	118	4.5	109	3.2
V	201	10.5	199	7.0
C	175	7.2	178	8.0

The reports of the observers may be summarized as follows:

R. The color began to lose in chroma very quickly. The outer edge of the circle paled out first, the paling gradually approaching the center. There was a little hue-change; the *R* faded slightly towards a very poor *O*. The resultant gray was a little lighter than the stimulus-color.

O. A thin gray film gradually thickened over the surface of the color. This caused the chroma to become poorer with the progress of adaptation. The hue changed slightly towards a *Y*, very poor in chroma.

Y. The hue faded away under the gray film until no color was perceptible. There was no hue-change.

YG. There was a hue-change to *Y*, very poor in chroma. The final stage was somewhat darker than the color.

G. The hue changed to *Y*, very poor in chroma. The gray film

very quickly obscured the surface of the color, only a greenish gray remained for some time at the last stage of adaptation. Slightly darker than the stimulus-color.

BG. There was no perceptible hue-change, only a fading out to a very pretty gray. The film began to come in from the edge of the disc, gradually approaching the center of the color. The gray field was a little darker than the stimulus-color.

V. The hue changed to *R* and then to *B*. The resultant gray was lighter than the stimulus-color.

B. There was no perceptible hue-change in this color. In every respect it faded out like the *BG*.

C. Gradually faded out to a *B*, very poor in chroma. This state persisted for some time. Then the field was almost of the same brightness as the stimulus-color.

It soon became evident that this experimental arrangement was unsatisfactory. The observers complained of eye-movement in almost every observation; and the difference in illumination from day to day influenced the adaptation-time considerably. Further, there is a marked difference between the adaptation-times of the two observers for certain colors, especially *R* and *G*. It was therefore necessary to secure better conditions of observation.

Part II.: Foveal Adaptation at Medium Intensity
Controlled Series

Procedure.—The experimental work was done in a dark room. The apparatus and stimuli were those described in our account of the preliminary series. The illumination was produced by three 75-watt, 110-volt, Edison-Mazda daylight bulbs; it was equal to 212.4 c. p. at a distance of 2.5 m. from the stimulus-color and a distance of 1 m. above and a little in front of *O*. The current was supplied by a D. C. motor in the university service.

In order to avoid eye-movement, as far as possible, the following precautions were taken. (1) A time-recording apparatus was constructed, to register accurately the time of every change in visual sensation. This made it needless for *O* to speak during an observation. The recording apparatus consisted, first, of a kymograph, set to revolve once in 3 min. A double electrical contact-marker worked over the smoked drum. To one marker was attached in circuit a large seconds-pendulum, while the other marker was in circuit with a telegraph-key operated by *O* in reporting the sensory changes. (2) A comfortable head-rest was fastened to the table in front of the stimulus-frame. With this arrangement, *O* could hold the head in a constant position during an observation.

O adjusted the head-rest, covered one eye with a black velvet eye-shield, and made ready to fixate the stimulus when the

light was turned on. The instructions were as follows: "When the light is turned on, fixate the center of the colored disc, and hold it if possible without moving the eye. If the eye does shift from its fixation, bring it back to position as quickly as possible. When the light is turned on and you have fixated, press the telegraph-key five times in rapid succession. Every time the eye moves in any way during fixation, press the key once. For every color-change of any kind, press the key twice. When the color fades out, or when it reaches a point where it shows no further change, press the key again five times. You will be asked to give an introspective report of these changes at the end of every observation."

Results.—The results are given in summary for the different observers and stimuli.

Observer S. Stimulus *Y.*— An after-image came very quickly. The mist appeared from the outside, causing the color to become less saturated and much lighter. As the mist grew thicker, there was a tendency for it to fuse in the border before the center became solid; thus the center was the last part to disappear. The cloud was much lighter than the background.

Stimulus *BG.*— The after-image was noticed very quickly. Soon the cloud gathered over the surface of the color like a fog or mist. This caused the color to be much less saturated. For a little while I could see the color through the fog as if it were behind the mist. The cloud which covered the color was darker than the stimulus-color.

Stimulus *B.*— A soft white film began to form over the surface; at about the same time I observed the after-image; in fact, the after-image and the fog could be clearly recognized as one. The after-image was merely superimposed over the color, and appeared to stand out from the color. Where this haze fell on the outside edge of the stimulus-color and on the bordering background, it took on the nature of the after-image; where it was on the stimulus-color it was a beautiful white fog or mist. As this mist or fog gradually grew thicker, the color slowly lost in richness of chroma without any hue-change. The resulting gray was of the same shade as the background.

Stimulus *R.* The gray film was peculiar in this color. It came in from the outside. The color appeared very dark through it. The mist was not smooth, but rough, very much like hazy clouds on a summer afternoon. It never had a smooth texture, like the blue, yellow or orange. At times I noticed shooting phenomena from the outside, like smaller clouds suddenly moving across the surface of the disc.

Stimulus *V.*— The cloud suddenly came in from the outside, at about the same time as the after-image was noticed. It was very hazy at first, but slowly thickened to a very dense mist. The color did not seem to change in hue in any way, but the haze seemed to thicken and conceal it from view. The cloudy gray was somewhat darker than the background.

Stimulus *O.*— The after-image was seen very quickly, bordering the color almost all the way around; with this came a very peculiar cloud, which seemed to be a part of the after-image, extending over the color, hiding the color. For a while the view resembled the sun behind a luminous fog.

Stimulus *C*.— The color became almost a bright red behind the thin misty cloud, when the cloud would move a little to one side. The after-image and the cloud could be recognized as the same surface, floating over the surface as the eye moved. The cloud was smooth and not rough like the red. There were shooting phenomena as with the red.

Stimulus *G*.— The cloud appeared about the time the after-image was observed. When the haze became very thick, there was noticed a little yellow mixed with the misty surface. This was probably a hue-change of the stimulus-color. The cloud in the beginning seemed to move in from the outside, growing thicker towards the center.

Stimulus *GY*.— The cloud appeared as if in front of the stimulus-color in the form of a light hazy mist. It became thicker and thicker until the color was obscured. The color could be seen for a considerable time through the fog; as the mist grew thicker, the color appeared to lose in richness of chroma, until only a gray of about the same brightness as the background remained.

Observer Sh. Stimulus *Y*.— An after-image came very quickly, which extended all the way around the stimulus-disc. The after-image was at first very poor in chroma. Almost at the same time a whitish film began to form over the surface of the color. As it thickened, it appeared to be of the same brightness as the background. This gray surface began as a very thin sheet moving over the surface. At times it appeared as threads or filaments, stretching across the surface, as if they were in motion; these thickened into a solid uniform surface.

Stimulus *BG*.— There was a short interval of time before the after-image was seen on the border around the stimulus-color. The misty haze about the same time was observed to move inward over the surface of the color. The film soon thickened in texture so as to obscure the color.

Stimulus *B*.— The after-image began to show very quickly around the edge of the stimulus. The haze began to form almost as quickly, and thickened into a very white misty film. This stood out away from the color, and at no time did it appear to be a part of the stimulus-color. The color gradually lost in richness of chroma as the film thickened. When the cloud had taken on a filmy or sheet-like appearance, it was very easy to see that it was an extension of the after-image over the stimulus-color, as a misty white luminous fog. It looked very much like a thin sheet of tissue-paper spread over the color; the film at all times was in front of the color and not a part of the color. The whole adaptation-process for the blue was a fading out or disappearing of the color behind the fog.

Stimulus *R*.— The after-image appeared very quickly as a bluish-green border on one side of the disc. At first it was pale, or washed out. but it gradually increased in chroma until it was very rich. At the same time a haze began to form over the stimulus-color, which gradually grew thicker. The haze appeared to come from the outside of the disc and to settle in front of the color. This haze for the red was a rough, uneven mist or fog; not smooth in texture like that of the yellow or orange. The hue showed a remarkable change during the course of adaptation. It changed almost as soon as the haze began to form over the color into a red-orange, then to an orange of very poor chroma, and then to a very dirty, dingy brown. This brown was persistent for some time; finally it changed into a gray surface which was somewhat darker than the background.

Stimulus *V*.—A very pale, faded after-image soon appeared from

behind the stimulus-color, scarcely visible on the gray surface surrounding the stimulus. The cloud or fog seemed to float in from the side of the color towards the center; not so pronounced as with the red. The film at first was thin and more solid than with the red, but not so smooth in texture as with the blue or orange. (In three observations there was a slight hue-change towards blue, the red going out first. In two there was only a gradual fading out of the color into the fog).

Stimulus *O*.— The pale blue of the after-image, which was seen almost from the first, soon increased in chroma until it was a rich, well saturated blue. At the same time a beautiful hazy mist was gradually thickening over the color. This film appeared very much like the sun behind a thin fog. An interesting phenomenon occurred with a shift of the eye. There could always be observed two distinct discs in the visual field; the one was the stimulus-color, and the other was the film or fog in front of the color. These two discs, when not superimposed, the cloud upon the stimulus-color, always gave three parts of a circle. The part of the luminous fog which was upon the background showed a very rich after-image. The segment of the stimulus-color which was uncovered by the fog, caused by a shift of the eye, was a rich bright color, of the same hue as the stimulus-color; its brightness always depended upon the brightness-value of the background with reference to the brightness of the stimulus-color. The third portion was that part of the stimulus-color upon which a part of the cloud was superimposed. This was a beautiful gray, in which no color could be perceived in the final stage of adaptation.

Stimulus *C*.— The after-image was slow in appearing. The cloud effect was generally noticed as soon as the after-image. A change in brightness is always noticed as the filmy surface begins to form over the color. At this point the observer can always tell when adaptation is really progressing rapidly, because the color begins to pale out very rapidly. The luminous fog was always smooth, gradually thickening from a filmy surface to a veil very much like a thin sheet of paper spread over the surface of the color. This finally became so thick in texture that the color was obscured.

Stimulus *G*.— The cloudy mist began to form over the surface of the color by floating in from the outside towards the center of the disc. This movement and the after-image came at the same time. This haze grew thicker, which caused the color to become less and less saturated until only a gray surface was left. Generally there was a slight change in hue towards yellow.[17] The yellow would appear as if it were in the foggy veil which floated before the stimulus-color.

Stimulus *GY*.— The after-image began as a very pale, washed-out carmine. It extended all the way around the stimulus disc. With an increase in the richness of the chroma of the after-image also came a thickening of the hazy film which formed with the after-image. The cloud was apparently of the same brightness as the background. In nearly every case the green was observed to adapt out first, leaving a dirty, muddy surface which remained as a stubborn color for a considerable time.

Observer L. Stimulus *Y*.— Soon a film formed over the surface of the color, which did not appear uniform at first, but thicker in spots. The color began to lose in saturation from the first, changing its hue to a tan or very light unsaturated yellow. The haze finally grew so thick that the color was obscured from view.

[17] This was no doubt due to the fact that the Milton-Bradley *G* contains a slight amount of *Y*.

Stimulus *BG*.— The after-image and the cloud-effect appeared very quickly and at almost the same instant. The fog was very light and fluffy. It stood out in front of the color. The hue of the color gradually faded away behind this white mist as the fog became thicker. The final stage of adaptation gave a gray surface equal in brightness to the background.

Stimulus *B*.— The after-image was quickly observed; at about the same time the misty cloud spread quickly over the surface of the color. The fog was almost pure white and appeared to fit the stimulus-color. It seemed to stand out away from the color. This mist gradually thickened into a thick cloud which hid the color of the disc.

Stimulus *R*.— The cloud floated in over the surface of the color from the outer edge of the disc. It was not smooth in texture, but somewhat rough and uneven. The red disc changed at first into a kind of orange, then to a yellowish color of very poor chroma, and finally faded out to a dark gray.

Stimulus *V*.—The after-image appeared very bright from the beginning. The misty haze is somewhat darker than the background. It gradually thickened over the color. It is difficult to tell the final gray, when adaptation is complete, from the violet when it is very poorly saturated.

Stimulus *O*.— The haze over the color and the after-image around the stimulus appeared at the same time. The cloud thickened and obscured the color from view. For some time the color could be seen as if shining through the fog. The final gray was of the same brightness as the background.

Stimulus *C*.— The after-image was at first dim and slow in appearing. The cloud was very thin at first and somewhat darker than the background. The color gradually paled away in the fog.

Stimulus *G*.— A very weak after-image appeared at first, which gradually increased in chroma. A misty cloud came in from the outside of the color towards the center. This haze increased in thickness until it obscured the stimulus-color. It is of the same brightness as the background.

Stimulus *GY*.— Cloud-effect appeared very quickly. This caused the color to become very much less saturated, as if a thin sheet of white paper had been suddenly spread over the surface. The mist thickened so as completely to hide the color. The final gray is the same as the background.

ADAPTATION TIMES IN SECONDS

AVERAGE OF FIVE OBSERVATIONS

Obs.	S		Sh		L		
Color	Av.	M.V.	Av.	M.V.	Av.	M.V.	Av. of Av.
R	84.8	7.0	91.4	7.9	98.0	6.4	91.4
O	88.2	7.4	92.2	5.4	92.4	11.1	90.1
Y	97.6	5.9	90.8	3.8	98.8	7.4	95.7
YG	62.0	6.4	63.2	3.4	58.4	7.5	61.2
G	55.2	1.3	58.4	3.5	55.0	3.2	56.2
BG	42.0	5.2	39.8	4.2	39.2	3.0	40.3
B	125.0	7.2	107.2	12.6	128.4	4.9	120.2
V	82.4	2.4	84.0	3.2	78.0	5.6	81.3
C	73.2	6.2	73.4	7.3	75.4	6.9	74.0

The curve for the times of adaptation in sunlight differs considerably from the curve for the same colors at medium intensity. *R* is relatively shorter at medium intensity, while *B* is slightly longer than *B* in sunlight. On the other hand, the *Y* and *G* regions of the two curves take almost opposite positions. There is not only a rapid rise in the *Y* region for medium intensity, but there is a shift of the shortest time to the *BG* region. This difference might evidently be due to a difference in the chroma of the *Y* and *G* regions, caused by the two sources of illumination.[18] We therefore determined the chromatic limen of observer Sh for every color under both sources of illumination.

Procedure.—A color-mixer was placed in the window, on the table used in the preliminary experiment, so that the discs should revolve in a horizontal position. The disc was made up of a black-velvet paper, a good white, and a colored paper. The relative proportion of the brightness-value of the color was kept constant by varying the black and white discs as the color component was increased or decreased. The method of limits was employed (five series). *O* did not see the disc until it was in motion and then only long enough to make a judgment. The experiment was repeated in the dark-room with the illumination used for medium intensity, and also in diffused daylight.

The *results* are shown in the following table:

CHROMATIC LIMENS

COLOR	SUNLIGHT		DARK ROOM ILLUMINATION		DIFFUSED DAYLIGHT	
	Av.	M.V.	Av.	M.V.	Av.	M.V.
R	18.0	3.6	22.0	6.4	19.3	1.6
O	34.0	3.8	20.0	6.2	8.0	1.3
Y	55.0	6.1	18.0	3.8	19.7	2.5
YG	46.0	4.3	28.0	5.2	23.0	3.6
G	37.0	4.3	32.0	4.3	22.2	2.3
BG	30.0	2.2	40.0	5.4	19.0	1.6
B	12.0	3.2	12.0	1.1	11.0	1.6
V	22.0	4.6	30.0	3.6	33.0	6.3
C	26.0	6.3	25.0	4.1	13.3	3.1

[18] The gray background upon which the stimulus-color was mounted, and which tested free from color under the daylight electric-bulb illumination, gave the following composition when placed in direct sunlight illumination: Black 215°, White 125°, B 12°, and G 8°.

There can be no doubt as to the effect of chroma on the time of adaptation in direct sunlight, and in illumination used in the dark-room experiment. The proportion, through the whole series of nine colors, runs in the right direction. Wherever the adaptation-time is long, the chromatic limen is small, and conversely. For diffused daylight, there is a slight variation; *R* and *V* are out of proportion. This exception may be due to the fact that *R* and *V* are the dark colors; since the illumination was rather weak, the limen would tend to be high. For all the lighter colors there is a close approximation to the law stated above, that where the time is high the limen is low. We shall later find that the same thing holds for spectral colors; chroma is the principal factor which influences the time of adaptation, with a possibility of intensity as a minor factor.

Part III.: Pure Spectral Colors

We proceed to describe the course of foveal adaptation to pure spectral colors, and to compare the behavior of these colors with that of surface-colors. We were careful to present the spectral color to *O* under controlled conditions. The wavelength of the spectral band producing the stimulus was determined before every observation; the intensity of the light producing the spectral color was always known; and the amount of light entering the eye was kept constant by the use of an artificial pupil.

Procedure.—The apparatus consisted of a remodeled spectrophotometer.[19] In addition to the original equipment of the apparatus, we inserted a field in front of the lens, so that any desired width of spectral band could be obtained in the form of a circular disc of color. An artificial pupil, 2.5 mm. in diameter, was placed 14 cm. in front of the stimulus field, and 2 mm. from the eye. A small projection-lantern, burning a 200 w. 115 v. Mazda nitrogen-filled daylight bulb, illuminated the spectroscope to produce the spectral colors. Before the lantern was placed a plate of thin and very finely ground glass to cut out the image of the filament in the light-bulb. The amount of light that entered the collimator of the spectroscope was 61.4 c. p.

The observations were made in the dark-room. All the light for illuminating the spectroscope was thoroughly concealed, so that only the color through the artificial pupil could be observed. A head-rest was set in front of the spectro-

[19] Manufactured by the Cambridge Scientific Instrument Co.

scope.[20] All changes in color-sensation were recorded in seconds by means of the apparatus described in Part II. *O* was given 15 min. of dark-adaptation before beginning the experimental work, and a period of 5 min. was allowed for rest between observations. Since monocular vision was employed, and the eyes were alternated from observation to observation, there was a period of at least 10 min. before an eye was restimulated.

For stimuli, circular fields were cut from the spectral band having the following wave-lengths; *R* 740-770 $\mu\mu$, *Y* 545-575 $\mu\mu$, *BG* 495-525 $\mu\mu$, *B* 440-470 $\mu\mu$, *V* 405-435 $\mu\mu$. The field was 6 mm. in diameter, and was just large enough to fall well within the macula lutea. The constancy of the light could be tested at any moment by means of a switch which turned the current through voltmeter and ammeter.

The *results* appear in the following table, as averages of five observations.

ADAPTATION TIMES IN SEC.

OBS.	SH.		S		Z		F		L		Av. of Av.
Color	Av.	Mv.	Av.	Mv.	Av.	Mv.	Av.	Mv.	Av.	Mv.	
R[21]	102	4	103	6	110	9	98	7	109	13	104.4
Y	166	10	164	10	180	21	Failed		166	7	169.0
BG	119	9	126	10	132	6	164	16	117	3	131.6
B	103	6	104	2	120	4	121	10	90	6	107.6
V	69	8	69	9	78	4	90	6	77	6	76.6

Observer F failed to get complete adaptation for *Y* at this intensity. In explanation we can only state that she had had less practice than the other *O*'s when she began the observations. All the others reported complete adaptation for all stimuli used.

The curve of the adaptation-times rises from *R*, reaches its maximum at *Y*, and gradually descends to *V*. This is almost an exact figure of the brightness-curve for spectral colors.[22]

[20] It was found at the beginning that any movement, even that of winking the eye, influenced the time of adaptation. The head-rest enabled the observer to hold the head in one position, leaving only steady fixation to be attended to. After a little practice with the artificial pupil, a good observer has no trouble in holding an observation period, often without even winking the eye.

[21] A psychological *R* was made up from gelatins. It was composed of *R* and *B* gelatins so combined that no hue-change was observed during the process of adaptation. The time of adaptation was: Sh 100, Mv. 3.2; S 104.2, Mv. 2.1; Z 104.9, Mv. 6.4; L 98.9, Mv. 11.2. F did not observe in this part of the series.

[22] W. de W. Abney, *Researches in Color Vision*, 78-111.

BOSTON UNIVERSITY
COLLEGE OF LIBERAL ARTS
LIBRARY

Hence we may have recourse to two factors, to explain the difference in the curves for surface-colors and for pure spectral colors: brightness and chroma. To test the first factor, we equated all the colors for brightness-value and then repeated the series, with the following result:

ADAPTATION TIMES IN SEC.

COLOR	SH.		S		L	
	Av.	Mv.	Av.	Mv.	Av.	Mv.
R	96	5.4	82	4.5	105	3.1
Y	115	6.6	120	3.0	130	6.9
BG	97	5.5	100	6.7	96	4.3
B	59	3.1	68	6.5	65	1.1
V	68	3.1	52	5.0	68	2.3

It is clear that an equation for brightness does not altogether remove our difficulty. To test the second factor, namely, the possible effect of chroma, we arranged a series of experiments as follows. Three color-fields were used: 4.5 mm., 3 mm., and 2 mm. in diameter. Since *V* has the lowest adaptation-time, a stimulus of this wave-length and brightness was set up on a second spectroscope. White light was added to another color in the spectrophotometer until the observer judged the two colors equal in chroma. When this point was reached, the observer was given 15 min. of dark-adaptation, and then observations were made with the three fields; the same colors were employed as in the first part of this experiment. The following are the averages for 10 observations by every observer on every field:

3MM. FIELD

OBS.	SH.		S		Z		F		L	
Color	Av.	M.V.	Av.	M.V.	Av.	M.V.	Av.	M.V.	Av.	M.V.
R	47.2	5.4	63.4	3.3	48.0	6.0	62.4	4.8	67.0	9.2
Y	47.2	3.4	66.4	2.3	48.2	5.1	62.8	9.6	63.8	5.4
BG	47.2	2.6	65.2	7.4	46.0	4.4	60.5	3.1	74.2	4.8
B	41.8	1.8	57.8	5.4	45.6	3.4	61.9	3.1	70.0	2.8
V	44.4	2.4	63.0	4.4	46.2	4.2	62.6	8.2	67.8	3.4

4.5MM. FIELD

R	64.4 6.7	57.8 3.5	59.2 4.6	64.4 5.3	55.0 9.6
Y	46.0 4.4	75.8 5.0	46.0 4.0	50.2 1.9	74.2 6.5
BG	47.4 6.8	73.4 4.5	43.6 3.7	89.0 1.2	80.8 3.8
B	54.6 6.8	63.4 8.8	46.6 7.1	71.6 7.9	67.0 6.4
V	46.0 3.2	63.8 2.6	44.8 1.8	106.0 8.8	64.4 8.6

2MM. FIELD

R	50.6 5.7	62.0 4.8	46.2 3.8	55.1 6.3	64.2 10.6
Y	62.6 1.9	59.4 8.3	53.8 3.8	61.2 6.8	64.8 7.4
BG	47.8 7.0	56.6 10.0	50.6 5.9	70.0 10.2	65.0 5.2
B	52.8 9.8	69.2 3.0	55.0 4.4	51.1 7.1	63.8 5.6
V	57.0 2.0	58.6 8.5	53.4 4.7	52.4 6.4	67.2 14.1

The introspective reports may be summarized as follows:

Observer Z. *Y.*— After about 30 sec. I observed a thin filmy cloud which began to form on the rim of the circle. The cloud was darker than the *Y*, and contained some color. This film became thicker and darker until there was no color left. The *Y* under the film always became brighter, paler, and gradually took on a washed-out appearance.

R.— The red began to get lighter. The hue began soon to change to a very good orange. This began about the time a thin film was forming over the color. The filmy surface gradually grew darker and lost color, until a good gray surface covered the color. At the same time the hue changed from *O* to *Y*, and then finally to a gray.

G.— The green began to fade out to a *Y* as soon as it began to lose chroma. The loss of chroma began along the border of the colored field. I could recognize the after-image, when the eye moved, as being a portion of the filmy surface over the color. The film was a soft white cloud. The *Y* then faded out to a very good gray.

B.— The whole surface began to brighten up together with a reduction in chroma. There was no change in hue. The chroma gradually faded out to a good light gray. This process was like a cloud gradually thickening so as to obscure a color beneath it.

V.— The *V* began to lose chroma very quickly; at the same time there was a change in hue; first the red faded very quickly to a certain degree, then the blue, and finally both faded out together. This loss of chroma was again in the form of a white film collecting over the surface of the color. Began on the border of the field, and then spread to the center, all the time growing thicker until the color was obscured.

Observer S. *Y.*— There was no change in hue. In the beginning of observation there appeared a light ring around the edge of the stimulus-color due to contrast. The color gradually paled out to a good light gray surface.

R.— The red went immediately to a very pretty orange, and from this to a yellow. A dark gray cloud appeared in the center of the disc. This gradually spread out over the surface of the color, and covered the whole disc.

G.— There was no hue-change; only paled out to a good gray surface. The final stage was almost a white. The cloud first appeared in the center of the disc and spread over the whole surface of the stimulus-color.

B.— The blue did not change in hue, but lost in chroma, until a very pretty gray surface remained. The color gradually grew lighter as it lost in chroma. There appeared a gray cloud in the center of the disc which spread over the entire surface.

V.— The hue of the violet changed, first to blue, losing the red; then the bluish-violet lost in chroma, until a very dark gray surface remained. The cloud appeared in the center of the disc and spread over the surface of the color.

Observer Sh. *Y.*— A whitish film began immediately to collect over the surface of the stimulus-color. It appeared to be a little brighter than the color. The film gradually grew thicker until it obscured all the color, leaving only a light gray surface. There was no perceptible hue change.

R.—The red went through a color-change, first to orange, then through *O* to yellow, and finally to a dark gray. A film was observed forming over the surface of the color at the same time it began to change in color. The film began to form in the center of the stimulus-disc, and gradually spread outward, covering the whole disc.

G.— There was a change in brightness at the beginning. A bright ring was noticed around the edge of the field. This extended inward towards the center very quickly. Then a light film began to form over the surface of the color. This gradually grew thicker until all color was obscured. There was no hue-change.

B.—There was a decided change in brightness. A light film formed immediately over the color. This grew thicker until all color was obscured. The resulting gray is almost a white.

V.— There was a change in hue towards the blue. The red faded out first; at the same time there was a loss in chroma, by a thin cloud forming over the surface. A very dark gray finally formed over the color.

Observer L. *Y.*—The color became a little lighter, and lost in chroma until it became a light gray. There was no hue-change.

R.— The *R* changed to *R-O*, *O*, *Y*, and finally to a dark gray.

G.— There was no hue-change. A cloudy mist began to form in the center of the circular field, and spread over the entire surface, which decreased the color in chroma until it was completely faded out.

B.— There was a gradual loss in chroma; no hue-change. A cloud-mist formed immediately over the surface of the color, which gradually thickened until no color was noticed. The final stage was almost white.

V.— There was a change in hue to a violet-blue color. The red almost faded out first; then the color was poor in chroma. The final stage was a very dark gray.

Observer F. *Y.*— The color grew paler by losing in chroma. There was no hue-change.

R.— There was decided hue-change in this color. It went to *O*, then to a very poor *YO*, which reminded me of a dirty *Y*. Finally lost in chroma, until a gray resulted which was darker than the color.

G.— The *G* lost in chroma by paling away. No hue-change.

B.—Same as the green.

V.—The *V* changed to *R* and then to *B*, all the time losing in chroma.

Since a chroma-match between two colors that differ in hue is only a relative matter, there will always be some variation. So far, therefore, as time of adaptation is concerned, we have every reason to be satisfied with our results. The averages for the 3 mm. field are, it is true, more uniform than those for the other two; the 4.5 mm. and the 2 mm. fields show slight fluctuations. But no variation is as great as with the brightness-equation.

From the introspective reports we find that there is no change in hue except for *R* and *V*. All the other colors which we used faded away steadily to a gray field. The adaptation-process makes its appearance in the form of a film or mist over the color. This causes the color to become less and less saturated, exactly as was the case with the surface colors. When there is a shift of the eye from fixation, *O* always reports three continuous portions of the field: a crescent of after-image; on the opposite side to this, a crescent of stimulus-color, very rich in chroma; and between the two crescents a white or gray surface over the stimulus-color.

ADAPTATION AT HIGH INTENSITY

It was found, in the first series with spectral colors, that foveal adaptation would not proceed to a neutral gray when the light that entered the collimator of the spectroscope was raised to more than 60 c. p. At the same time three important facts were observed in regard to foveal adaptation at high intensity. (1) All our records show that there is a tendency for eye-movement to increase very rapidly at the end of a fixation-period, in some cases as much as 4 or 5 times the amount at the beginning of a period. (2) Our *O*'s report that adaptation is always very rapid at the beginning of a period, no matter how high the intensity of illumination may be, provided it does not produce a blinding glare.[22] (3) There is always a good deal of pain in fixating a bright colored light, even with foveal fixation, when the intensity passes much beyond 100 c. p. This pain may be noticed even as long as two or three days after the observation. It appears, then, that when a certain intensity is reached the eye moves so frequently, during the latter part of the period, that the process of adaptation can not be completed. The pain is probably caused by the excessive contraction of the pupil in response to the reflex set up by the very intense beam of light striking the fovea through the artificial pupil. In the following experiment we tried to overcome these difficulties, and to study the process of adaptation at high intensities.

[22] See P. G. Nutting, *Trans. Illum. Eng. Soc.*, xi, 1916, 943.

Procedure.—The exposure-apparatus was a projection-lantern which burned a carbon 13 mm. in diameter, carrying a 30 amp. (125 v.) D. C. current. The lantern was mounted with a projection-lens of 2 in. diameter. In front of the projection-lens was placed a short-focus lens to bring all the rays of light to a pencil-focus. The actual amount of light focussed by the lens was about 6,200 c. p. The head of the observer was held in a steady position by means of a head-rest. The focus of light was adjusted to fall within the eye. Colored gelatin-filters (Eastman Kodak Co.) of known wave-length and transmission-power were used for stimuli; a filter of any desired color could be immediately fitted into one side of a pair of goggles, made to hold the filter securely. In the other side of the goggles was fitted a dark piece of cardboard, to exclude all light from the unstimulated eye. A foveal comparison-light was set up in such manner that any spectral color, from the intensity afforded by the lantern to the lowest possible intensity, could be obtained immediately by shifting the lever of a rheostat. We measured the actual intensity of the light that entered the eye through the filters by the following method. A 500 watt Mazda lamp was screened to match daylight (acetylene flame and No. 79 filter) and was set up at one end of the photometer bench This procedure was adopted because the photometry of a carbon arc for this purpose is not as precise as the photometry of an incandescent lamp. A flicker photometer and a 20 c. p. comparison lamp were set up at the other end. When necessary, neutral-tint filters were used to reduce intensities.

For determining the transmission, a setting was made with the filter screening the 500 watt lamp, and then a setting was made without the filter; the ratio of these two readings on the candle-power scale gave the transmission. The following is an average of several trials:

Color	Filter No.	Transmission	x C.P. of Lantern=About	
R	70	7.6%	471.2	c.p.
Y	73	35.9%	2225.8	"
BG	75	18.6%	1153.2	"
B	48	19.5%	1209.0	"
V	76	1.6%	99.2	"

All work was done in the dark-room. *O* adjusted the filter to the eye, placed his head in the head-rest, and awaited the stimulus-light. The instructions were: "Fixate the bright circle of light until all color disappears. When you no longer see any color, remove the goggles, fixate the spectroscope, and describe what you observe." The following are the average adaptation-times in sec. for four observers:

	Sh	92	The hue quickly changed to *O*, then to *Y*, very poor in chroma. The *Y* persisted for some time. Finally all hue disappeared.
	Z	109	
R	S	120	
	F	100	The foveal comparison-color was pure gray, much lighter than the stimulus-color.
	Av.	105.3	All intensities were gray.
	Sh	165	There was no change in hue. The color gradually faded away.
	Z	177	
Y	S	150	
	F	175	The comparison light at all intensities was almost white; no trace of color.
	Av.	166.8	
	Sh	120	No change in hue.
BG	Z	112	
	S	98	Very light gray. No color at any intensity.
	F	130	
	Av.	120	
	Sh	240	No change in hue.
	Z	182	
B	S	155	Very light gray for all intensities.
	F	148	
	Av.	181.3	
	Sh	200	There was a change in hue; the *R* apparently weakened first; then there was a noticeable decrease in chroma for the *B*. The *R* disappeared, leaving only a very weak unsaturated *B*, which slowly faded into a dark gray.
V	Z	183	
	S	185	
	F	220	The foveal comparison light showed no trace of color for any intensity.
	Av.	197.0	

We were able to get complete foveal adaptation for all *O*'s with these intensities, as for all lower intensities. After a long fixation, beyond the point where adaptation was complete, we were able even to raise the comparison-light somewhat higher than the standard, and still had complete adaptation in the fovea. However, long fixation with a bright light is very painful; the pain even persists for two or three days as an ache or soreness within the eye.

It might be objected that the blinding glare of the positive after-image here influenced the judgment. We were, however, careful to test this factor before finally deciding upon the technique for the series. If the arc was fixated without the filter, there always resulted a blinding glare. When the foveal

comparison-light was fixated in this condition, there was only a slight change in brightness of the color, which immediately passed away. This was not true when a stimulus-color was fixated. The foveal comparison-light was always colorless, and so remained. It must, however, be small enough to fall within the foveal cup, and must not extend to the periphery of the macula.[24] Otherwise a more pronounced effect will be noted from glare.

Adaptation at high intensity is difficult to describe; but so far as we were able to observe the same general process takes place that has been observed throughout the earlier experiments of this study.

Conclusions

In general adaptation to direct sunlight, as well as under other conditions of adaptation, both to surface and to spectral colors, the fovea has a longer adaptation-time for color than any other part of the retina. This law aparently holds for high intensities as well as for medium and low intensities.

Chroma is the principal determinant of the rate of adaptation. Intensity is a second factor. The effect of both chroma and intensity has been observed with surface colors as well as with spectral colors.

The rate of adaptation is always faster at the beginning of a fixation-period, gradually decreasing, and becoming very slow at the end. The rate at the beginning also depends upon the chroma; the richer the chroma, the faster is the initial rate of adaptation. It gradually becomes slower later, and the whole process lasts longer than for a color poorer in chroma.

There is no difference in the experienced course of adaptation between a surface color and a pure spectral color. A color, under the process of adaptation, very quickly begins to decrease in chroma. This decrease is observed in the form of a light film which spreads over the color. The film continually thickens, until the color is obscured under the fog. The appearance of a color after adaptation corresponds in general to a mixture of that color with its complement. This relation was observed in nearly every observation. If the eye moved slightly from fixation, there were always reported three parts in the field: a crescent of the after-image, an opposed crescent of the stimulus-color, and an intermediate neutral gray surface.

Under suitable conditions, adaptation in direct vision can be carried to completeness at any physiological intensity.

[24] J. von Kries, *Z. f. Psych. u. Phys. d. Sinnesorg.*, xv, 1897, 327.

THE FOSTER-CHILD FANTASY

By Edmund S. Conklin of the University of Oregon

This study was designed to test the validity of the generalization made by the psycho-analysts that most or all children experience the foster-child fantasy, and to determine some further facts concerning the phenomenon. As a stage in the development of the "family romance" it is said that all or nearly all children conceive the notion that they are the children of much greater parentage. If this is so, especially as some think that it exerts much influence upon the conduct of the child, it is of great interest to students of childhood and adolescence.

The generalization seems to be well on the way toward acceptation. Abraham apparently considers it universal and sufficiently well demonstrated (1. pages 40-41). Adler refers to it as a not infrequent occurrence (2. page 206). Hall in his interpretation of Jesus' concept of divine parentage treats the foster-child fantasy as a very common experience of childhood (3. pages 251-2). Jelliffe is more daring and states that the family romance is a "universal phenomenon" (4. page 54), although he adds that any stage, and this could of course apply to the foster-child stage, may be "much abbreviated." White treats it as established in his introduction to psychoanalytic psychology (6.). Many Freudian writers, however, refer to or use the concept of the family romance with an air of certainty but without specific mention of the foster-child fantasy. Possibly they assume its existence as a part of the romance. Rank's study of the myth of the birth of the hero is most frequently given as reference (5.). Both Jelliffe and White quote Rank's presentation at length. And Rank presents the matter with apparent confidence in the conclusiveness of its demonstration. He also says that psycho-analyses are the only means of determining such traits (5. page 63).

Acceptance of the establishment of the foster-child fantasy as a common trait of childhood has hitherto depended upon the validity of conclusions from psychoanalyses, of psychoneurotic adults mostly, some of whom died long prior to the time of the analysis. That Rank's presentation so often quoted occurs in the course of a study of mythology should

not be permitted to lead to the supposition that it is an induction from his myth data. He himself says that it is a summary of the traits of childhood discovered by Freud "from his experience with the psychology of the neuroses" (5. page 63). Whatever may be thought of the validity of generalization from such data, it must be evident that here is an opportunity for checking the results of psycho-analytic work by use of another psychological tool, the questionnaire. In fact it is for just such work as this that the questionnaire is best suited, for a census of certain human experiences. If it is true that most children wonder whether "they can really be the offspring of their prosaic parents" and develop notions about their real parentage, then it should not have entirely disappeared from voluntary recall by mid- or even later adolescent years. Hence questions presented to adolescents under circumstances calculated to arouse a sober attitude of willingness to cooperate should produce other and valuable evidence concerning the frequency and nature of the foster-child fantasy and serve at the same time as a check upon the generalization made from psycho-analyses. Such is the nature of the study here presented.

With such a purpose, the questionnaire could be very simple and direct. After many preliminary experiments it was finally used in the following form:

It has been observed that some people during childhood or youth acquire the notion that their parents are merely foster parents. The purpose of this questionnaire is to determine how common such an experience is and some facts about it.

Your sincere effort to answer the following questions accurately will be appreciated.

Negative answers if true are just as valuable as positive.

Write answers in the space following each question.

1. Age at present.
2. Sex.
3. Did you ever have the experience of day dreaming or believing that you were an adopted or foster-child, that your supposed parents were not your real parents? (Answer yes or no).
4. Was it merely a day dream or did you believe it to be true?
5. Who or what did you think that you were?
6. If you can remember what caused such thoughts, please state it as clearly as you can.
7. About how old were you at the time?
8. Did these ideas lead you to any special acts or changes of conduct? If so, please state what.

9. If you believed yourself an adopted or foster-child, about how long did the belief last?

10. Has the belief disappeared? Tell why it disappeared if you can.

11. State below any remarks or information which may be of interest concerning the matter.

This was answered by students in scientific departments of the University of Oregon and by students in the two upper classes of eight different high schools in the same state. The college students wrote under the supervision of the writer or one of his colleagues in psychology. In the high schools they were written under the supervision of the superintendent or principal, people experienced in the handling of students and here selected for their interest in research. Several of them had formerly been students in the writer's department. In order to insure as nearly perfect uniformity in the presentation of the questionnaire as possible, printed directions were supplied to all who assisted in its presentation. Care was taken to make certain that the students understood that a serious purpose existed for the questions, which otherwise might have provoked frivolity, and by this means serious answers were obtained. Out of more than nine hundred returns only five bore any indication of insincerity. However otherwise this study may be attacked the author is confident that insincerity in its data is not one of its weaknesses.

Nine hundred and twenty one returns came to the writer. Of this number five were rejected because of apparent insincerity, ten because of failure to answer some important question such as age or sex, and two more were dropped because they were actual instances of foster children. Thus the total number of returns upon which this study is based became 904. Of these 640 were from the two upper classes in the high schools and the remaining 264 were college students, mostly freshmen and sophomores. Their distribution by age and sex follows:—

AGE DISTRIBUTION

Age	14	15	16	17	18	19	20	21	22	23	24	25	over 25	Total
Male	2	11	38	86	68	56	44	21	13	5	5	2	7	358 or 39.6%
Female	5	10	70	168	134	69	43	17	11	6	4	3	6	546 or 60.4%
Total	7	21	108	254	202	125	87	38	24	11	9	5	13	904

Socially this group might best be characterized as homogeneously American. Students of foreign born parents were few and students of foreign birth rare. They came as a

rule from homes of moderate to considerable culture, identified chiefly with commercial, agricultural or professional pursuits. Extreme poverty or utter illiteracy would rarely be found.

Frequency of the Fantasy

Question No. 3 was designed to discover the frequency of recallability of the fantasy and was worded to include with affirmative answers all degrees of belief or unbelief. It was answered by all of the 904 students participating in the study. The distribution of the answers was as follows, affirmative indicating recall of some experience with the fantasy and negative no immediate recall:—

	Male	Female	Total
Affirmative	87 or 24.3%	171 or 31.4%	258 or 28.5%
Negative	271 or 75.7%	375 or 68.6%	646 or 71.5%
Total	358	546	904

Age Distribution of Answers to Question No. 3

Age	14	15	16	17	18	19	20	21	22	23	24	25	over 25	Total
Affirmative														
Male	1	4	8	27	13	8	13	4	3	1	3	—	2	87
Female	1	2	22	50	41	20	14	8	2	5	2	2	2	171
Total affirmative	2	6	30	77	54	28	27	12	5	6	5	2	4	258
Negative														
Male	1	7	30	59	55	48	31	17	10	4	2	2	5	271
Female	4	8	48	118	93	49	29	9	9	1	2	1	4	375
Total negative	5	15	78	177	148	97	60	26	19	5	4	3	9	646
Grand total	7	21	108	254	202	125	87	38	24	11	9	5	13	904

It will be observed that the median for the affirmative group is the same as for the negative group. The likeness of these distributions indicates that there is no significant change in the frequency of recallability with age, that for the mid-adolescent years at least there is no rapid suppression. The older ones recall the experience as frequently as the younger. If this phenomenon continues on into the higher ages it would be out of harmony with the psychoanalysts' assertions concerning the disappearance of childhood fantasies from voluntary recall, but the data here for the upper ages is not sufficient for generalization. It merely suggests an alluring opportunity for further research. That 28%, however, of this group of more than nine hundred young people can immediately recall having experienced the fantasy does lend considerable support to the assertions of the psychoanalysts concerning its frequency.

Attitude Toward the Fantasy

Preliminary experimentation and inquiry revealed that some people could recall definitely having experience with the fantasy but merely as a day dream while others had actually believed themselves foster-children. For this reason question No. 4 was designed to bring out the frequency of the two groups, but it did rather more than was expected. It led to answers which revealed many gradations of belief. Some would not even admit that it was a day dream and used expressions of which a few follow:—"I merely wondered if I might belong some place else," "Just a casual thought," "Did not believe it to be true but thought of possibilities if it was," "Merely wondered," "Merely a random thought not developed to any degree or thought of later," "Just pretending," "Just a thought that I knew at the time wasn't so," "I liked to pretend my parents were foster parents." For the lack of a better term such answers were classified below under the heading, Casual Thought. Of those who answered that they believed their fantasy to be true some added qualifying phrases which also indicate gradations on the scale of belief but at the other end of the scale from the samples just given. Here follow some of the phrases in the belief group;—"I believed that it might be true," "I perhaps believed it true," "Day dream at first later believed it to be so." "Half believed it at the time," "I believed it or rather hoped it to be true," "Perhaps I did not fully believe I had foster parents but I doubted that my parents were truly mine." But most of the answers in this group leave no room for doubt for they are such phrases as "I believed it" or "I thought it to be true." The day dream group also indicates occasionally such gradations of belief as indicated by the following quotations;—"A day dream but at times was uncertain," "Merely a day dream but thought on it a good deal," "More of a day dream rather wishing I might be some one else's child," "..... wondering how it would feel to be a foster child," "Day dream but I nearly believed it to be true," "It was dreaming I suppose and yet I often thought that perhaps I had had a different father," "Day dream afraid it might be." Below are the frequencies of the answers in tabular form.

Frequency of Belief in the Fantasy
(Answers to Question No. 4)

	Male	Female	Total
Belief	17 (19.5%)	54 (31.6%)	71 (25.4%)
Day dream	58	101	159
Casual thought	12	15	27
No answer	—	1	1
Totals	87	171	258

From this it will be seen that all but one of those who answered question No. 3 affirmatively gave some indication of their attitude toward the fantasy. Twenty five per cent of all who could recall the experience recall it as being believed at the time. More than 19% of the males believed it and more than 31% of the females. Of all the 904 answering the questionnaire 7.8% recalled experiences of the fantasy and believed it, 4.5% of all the males had it and believed it, and so did 10% of all females.

Forms of the Fantasy

(Answers to Question No. 5)

Answers to this question indicated several degrees of development of the fantasy. Many returns merely said "adopted child" or "Never thought that far" or other words with like meaning. Such answers are grouped in the table below under the first heading as they seem to represent an incipient or undeveloped stage of the fantasy. Then there are those, of considerable number, who thought themselves foundlings of insignificant inheritance or destitute orphans who had been taken out of pity from an asylum. A few of these answered with the single word, orphan; but most gave some qualifying phrase or word which indicated socially inferior origin. These are all grouped below under the second head. A small number very clearly stated that they thought themselves children of parentage of a similar social status (third heading in the table). Another considerable group had developed ideas of greatness. Kings, princes, great wealth, great beauty, great intellectual powers,— all these figured in the concepts of their supposed true parentage. A few thought of themselves as strange, mysterious or supernatural beings, using these very adjectives on their returns, and they are so described in the table. It may be worth while to add that these few did not all come from any one community but from different parts of the state, thereby eliminating the possibility of collusion. Then there were those who recalled the fluctuating nature of the fantasy and report themselves as thinking of now one and now another type of person. Under Miscellaneous and Indefinite are presented those who gave such answers as "Kidnapped," "Traded by mistake," etc. making them otherwise unclassifiable. The frequency of occurrence of each group is indicated below as well as the percentage which the total of that group is of the entire number answering this question.

	Male	Female	Total
Incipient stage of fantasy(No clear concept)	47	75	122 or 54%
Orphan or foundling	7	26	33 or 15%
Parentage of similar social status	2	5	7 or 3%
Child of great parentage (Great wealth 20, royalty or nobility 11, miscellaneous 9)	8	32	40 or 18%
Strange, mysterious or supernatural being	4	1	5 or 2%
Many different characters	4	6	10 or 4%
Miscellaneous and indefinite	2	5	7 or 3%
Totals	74	150	224

Of the two hundred and fifty eight people whose answers to Question No. 3 indicated experience of the fantasy, two hundred and twenty-four or 87% answered this question. It was answered by 85% of the males and 88% of the females who answered No. 3 affirmatively.

Causes Offered for the Fantasy

The following table summarizes the answers to question No. 6 and in most instances the items are in the words of the questionnaire returns. The percentage figures indicate the per cent which the accompanying figure is of the total number of that class answering this question. (Totals here cannot be checked with totals in other tables because here some give more than one answer).

	Male	Female	Total
1. Suggestion: Books, stories, etc., and the hearing and seeing of actual instances of foster parentage.	30 (43%)	66 (46%)	96 or 45%
2. Dissatisfaction: Limitations of home, longing for wealth, discontent.	1 (1%)	9 (6%)	10 or 5%
3. Mistreatment (Actual or supposed): Harshness, partiality, teasing, fault finding, inconsistent treatment, desire for affection, lack of respect for parents.	19 (27%)	34 (24%)	53 or 25%
4. Peculiar Family Circumstances: Prolonged absence from parents, parental disagreement, great difference in children's ages, family quarrels, previous marriage of one parent.	2 (3%)	5 (3%)	7 or 3%
5. Accidental Causes: Parent misunderstanding child, misunderstanding of adult remarks.	1 (1%)	3 (2%)	4 or 2%

	Male	Female	Total
6. Activity of Mind (Instinct to think): Precocious philosophizing, mere play of imagination, much alone, nothing to do.	11 (16%)	16 (11%)	27 or 13%
7. Growth Stimulus: Early adolescent development, desire for the different.	1 (1%)	2 (1%)	3 or 1%
8. Absence of family resemblance, mental and physical.	3 (4%)	13 (9%)	16 or 7%
9. Miscellaneous: Praise with ulterior motive, self pity, lure of elsewhere or of the wild, mood, circumstances, desire for older brother, vivid dream.	7 (10%)	2 (1%)	9 or 4%
10. Unable to recall.	5 (7%)	13 (9%)	18 or 8%

Of those giving affirmative answers to question No. 3, 214 or 83% answered this question. Of these, 70 were males or 80% of the males answering question No. 3; and 144 were females or 84%. In only two instances is there a noticeable difference here between the sexes. Absence of family resemblances seems to be more significant for the girls and there is a similar preponderance of boys in the group called mental activity or the manifestation of the instinct to think. In passing it is interesting also to observe that all of the ten cases under dissatisfaction had ideas of greatness in their fantasies. This will be discussed further in the next section.

Relation of Fantasy Forms to Causes Given

Observing almost as many reporting notions of inferior parentage as there were of those reporting ideas of greatness, with other indications of developmental stages of the fantasy already mentioned, it seemed possible that a segregation of the causes which each of these groups offered might also be productive. The results of this segregation appear in the following table:—

	Orphan or Foundling Group	Ideas of Greatness
Suggestion	19 or 58%	12 or 30%
Dissatisfaction	0	10 or 25%
Mistreatment	5 or 15%	6 or 15%
Miscellaneous and Indefinite	9 or 27%	12 or 30%

The most conspicuous thing appearing in this comparison is the complete absence of dissatisfaction causes in the "orphan group" and its emphatic presence in the "greatness group." Mistreatment is about equally influential in the two forms of the fantasy, and suggestive causes are given much

more frequently for explanation of the fantasy by those in the "orphan group" than by those in the "greatness group." For help in grasping the meaning of these likenesses and differences reference should be made to the presentation of data concerning the forms of the fantasy. There will be found indications of stages of development, as was also suggested in the answers to the question concerning belief in the fantasy. Following this suggestion of a developing fantasy the table just presented would seem to indicate that mistreatment and suggestion first give rise to the idea of foster-parent-hood which early takes the form of orphanage or inferiority of some sort, perhaps assisted by feelings of helplessness or inferiority in the face of supposed ill usage or partiality for other children. Then with the continuation of the fantasy, growth and the vigor of approaching adolescence come ideas of greatness, doubtless again assisted by the suggestive causes mentioned, and reflection on these ideas of greatness might be the reason for the appearance of the alleged dissatisfaction causes in the "greatness group," the dissatisfaction thus being more consequence than cause. This seems much more likely than that the ideas of greatness should come first and with continuation of the fantasy the dissatisfaction disappear. Comparison with the complete table presenting all causes alleged will show all dissatisfaction cases are found in this table correlated with ideas of greatness, whereas only a small number of the suggestion cause cases and mistreatment cause cases are found here. All dissatisfaction cases in this study had ideas of greatness. Such a statement would be far from the truth for any of the other cause groups. It will be further observed that while the miscellaneous and indefinite group is a considerable percentage of the whole only a most improbable distribution of them, if more definite answers were obtainable, could alter the relationships between the groups.

Age to which Fantasy is Attributed
(Answers to Question No. 7)

The following table presents the ages which 240 of the 258 people who had recallable experience with the fantasy mention as being the approximate time of its occurrence:—

Ages	4	5	6	7	8	9	10	11	12	13	14	15	16	17	Indefinite or No Answer	Total
Males	1	2	5	5	7	5	20	5	10	3	7	3	2	2	10	87
Females	5	8	10	11	28	18	26	7	21	11	6	7	3	2	8	171
Total	6	10	15	16	35	23	46	12	31	14	13	10	5	4	18	258

It will be observed that definite answers were obtained from 93% of the group. Medians for both the male and the female distributions fall in the ten year group. This is undoubtedly too low because of the method used in tabulation. Some gave a definite figure, some said about a certain age and of course in these instances the figure given was used. But there were also many who answered in terms of an approximate range of years, as for example "About ten to twelve." Where this was done the tabulation was by the lowest figure given. Thus the actual measure of central tendency is higher than ten years, probably between ten and twelve years. Of course these are only rough approximations, but it must be remembered that most of these are approximations by young people not very far removed from the period in question, by young people still in their adolescent years (the median for the entire group studied being 18 years).

Effects of the Fantasy upon Conduct
(Answers to Question No. 8)

In this table are presented in summarized manner the answers to that question which sought to discover if any considerable number could recall influences of the fantasy upon their behavior, and in the event of there being such what the nature of the conduct effects might be. Of course the table is based upon the answers of those who had in response to question No. 3 indicated recallable experience with the fantasy.

	Male	Female	Total
No answer given	15 or 17%	27 or 16%	42 or 16%
No conduct effect recalled	55 or 63%	84 or 49%	139 or 54%
Some effect reported (See next table)	17 or 20%	60 or 35%	77 or 30%
Totals	87	171	258

It is worthy of note that this question was answered positively or negatively by 83% of the males and 84% of the females, or 84% of the group reporting experience with the fantasy.

The 77 people or 30% above who reported that the fantasy did have some effect upon their conduct, which they could recall, describe the effect in various ways but with sufficient clarity to make possible an effective grouping. It appears in the table following:—

Nature of the Conduct Effects	Number	Per Cent
Deliberative thought, seeking proof, meditation, etc.	26	34%
Depressing emotional reactions:—		
Melancholy, embarrassment, fright, etc.	6	8%
Domesticating conduct:—		
Expression of greater affection for parents (1), conscious imitation of parents (1), improved conduct (1), more considerate conduct (2).	5	6%
Alienating conduct (from parents):—		
Teasing, tormenting, deceitful, disobedient, revengeful, etc. toward parents (7).		
Stimulated independence of conduct, running away considered and carried out, or more self confident (9).		
Acting part of supposed real self (4)		
Reserved toward parents (4).		
Telling of the discovery (3).		
Seeking discovery by real parents (1).		
Deliberately acting in strange manner (1).		
Suspicious, sulky, saucy, snobbish, etc. (8).	37	50%
Miscellaneous (Poetic efforts, and disinclination to study).	2	3%
Positive but indefinite	1	—

The sub-topics in the table just presented are in most instances words and phrases taken directly from the questionnaire returns. In the recollection of these students the predominating effect of the foster-child fantasy upon conduct, or at least its conduct concomitant, is conspicuously that of alienation from the parents. This is of course in clearest harmony with the family romance generalizations of the psycho-analysts. The seeking of proof and meditative thought might be considered as a stage preliminary to the alienation in the fully developed phenomenon. So also the depressing emotional experiences reported. But the tendencies classed above as domesticating indicate a tendency in the other direction. Of course the writer is quite well aware that these may not be effects of the fantasy at all but merely growth concomitants, perhaps causing the fantasy. The writer is here merely seeking and presenting the opinions of his subjects.

As before, the returns in the "orphan group" and the "greatness group" were then isolated and the answers each gave to the question concerning conduct effects was tabulated according to the rubrics just used in order that any significant group peculiarities might again be revealed. The result was as follows:—

Effect Reported	Orphan Group	Greatness Group
Deliberative thought, etc.	6 or 18%	2 or 5%
Depressing emotion	2 or 6%	....
Domesticating conduct	3 or 9%	....
Alienating conduct	7 or 21%	13 or 32%
No conduct effect reported	13 or 40%	17 or 43%
No answer given	2 or 6%	8 or 20%

The great difference between the percentages of the two groups giving no answer to the question makes any generalizations from the other differences hazardous. The other differences suggest, however, a trend away from deliberative thought, depressing emotion and domesticating conduct (early stages perhaps of the fantasy) toward alienating conduct.

Duration of the Fantasy Believed
(Answers to Question No. 9)

People answering the questionnaire who had actually believed themselves to be foster children were asked to indicate as nearly as possible how long the belief continued. The following table summarizes their estimates:—

	Male	Female	Total	Per cent
No answer or too indefinite for use	4	8	12	17%
Very brief time (few minutes to few days)	6	11	17	24%
"Three months," "four months", or "few months".	2	5	7	10%
About one year	2	4	6	8%
About two years	1	13	14	20%
About three years	1	6	7	10%
About four years	..	3	3	..
About five years	..	1	1	..
"Several years"	..	3	3	..
About fifteen years (prolonged separation from parents).	1	..	1	..
Totals	17	54	71	

Some indicated in their answers that the belief was intermittent by the phrase "off and on" for the time specified. The long duration of the believed fantasy is a little surprising. Thirty five or 49% think the belief endured for a year or in some cases much more. It is also of interest that the question was answered in a specific manner by 83% of those reporting belief in the fantasy. Generalizations from these figures would, however, be unreliable because of inconsistencies in reporting. The experience was apparently spasmodic or recurrent with many; and in answer to this question some gave

an estimate of the period within which these recurrences came, while others gave merely the approximate duration of a single occurrence.

Disappearance of the Belief

The tenth question revealed that as a firm belief the fantasy had disappeared in all cases; although two or three report a lingering influence well described in the phrase of the one who said, "At times it seems to haunt me."

The second part of the question asked why the belief had disappeared, of course seeking the opinion of those answering. The answers to this appear in the following table:—

Reasons Given for Disappearance of Believed Fantasy

	Male	Female	Total	Per cent
Growth and development of intelligence, recognition of absurdity of belief, discovery that others had experienced the same thoughts.	9	24	33	46%
Parental intimacy (parental confidence, assurance and explanations and parental affection).	4	17	21	30%
Discovery of proof (Observation of family resemblances, discovery of records, etc.)	..	12	12	17%
Miscellaneous (One vague answer, one where aspects of belief failed to materialize).	..	3	3	..

The numbers in this table refer to the number of times each reason was mentioned. As some were mentioned more than once the numbers will not of course check with the other tables. The percentages are based on the number who believed their fantasy, that is 71. This question was answered by 83% of them in a definite manner. The others did not answer or said they were unable to do so.

Comparison with Results of Psychoanalyses

That inquiry of more than nine hundred young people should reveal twenty-eight per cent who could immediately recall experience with the foster-child fantasy supports the Freudians at the outset. To this number ought of course to be added all those who could immediately recall the experience, those who had it and with whom it is now beyond even mediate recall (instances of which with adequate proof have come to the

writer's attention aside from those here studied), and those in whom it may have suffered a psychoneurotic suppression. Of course such an addition cannot be made in terms of figures; but the mention of such cases indicates the minimal nature of the twenty eight per cent discovered by this study. Thus considered the number is sufficiently large to warrant the assertion that it is a common experience of childhood. So far the psychoanalytic generalization has been justified. And a more definite statement can now be substituted for Rank's that "many persons . . . very frequently remember occasions of this (foster-child) kind."

From psychoanalytic studies the foster-child fantasy was located in the pre-adolescent years (Freud's period of sexual latency or Jung's pre-sexual period). The two hundred and fifty-eight young people found in this study to have had recallable experience with the fantasy located it in the same period of development. The median of the answers given fell between the ages of ten and twelve years. The years eight to twelve inclusive covered 61% of the answers.

Taking Rank's oft quoted statement of the course of the family romance as the authoritative presentation for the psychoanalysts one finds in addition to the basic parental detachment motive of the romance the mention of two immediate causes of the foster-child fantasy, feelings of parental neglect or lack of affection and romantic literature. These two immediate causes have been amply verified by the results of this study. They were the two causes most frequently mentioned by these young people. But there has also been brought to light certain other supplementary causes, not mentioned by Rank, which in the minds of these subjects are of some importance. They are prolonged absence from the parents, marital infelicity observed by the children, precocious philosophizing, lack of companionship and the absence of mental or physical resemblance to the parents. The last is of peculiar interest not only because it is so common a topic of discussion before children but also because the subsequent development of family resemblances is several times mentioned in these returns as a cause for the decline of belief in the fantasy. One of those who had never experienced the fantasy remarked that "the reason I have never acquired such a notion is the fact that all my life every one has spoken of the great resemblance I have for my father."

Rank presents the fantasy as undergoing development from an early stage in which the child thinks of himself as an adopted or step-child to the higher stage characterized by expansive ideas, variously elaborated according to the child's adroitness

and the material at his disposal. This questionnaire study not only supports but also considerably elaborates the psychoanalytic conclusion. All through the returns there are indications of the developmental stages of the fantasy. Concerning the forms of the fantasy there were those, a large group, who had but the vaguest if any idea beyond the thought of foster-childhood, some saying that they had never thought further than that. Then there were those who reported thinking themselves as different characters at different times. The apparent relationship of the different form groups (incipiency, orphan, same social status, great parentage, supernatural being) suggests the same thing. The relationship of forms to causes carefully tabulated indicated the developmental interpretation of the fantasy as the only feasible explanation of the otherwise strange distribution of causes mentioned. The relation of the forms to the reported conduct effects pointed, if less clearly, in the same direction. Examination of the conduct effects indicated stages from meditation on a more or less fascinating idea to decidedly alienating conduct. Degrees of belief also appeared very clearly in the answers given. Even without knowledge of the psychoanalysts conclusions it would have been difficult if not impossible to have interpreted this data otherwise. And the conclusion is in harmony also with the known traits of childhood imaginative fancy as well as the growth of delusions in paranoia.

The forms of the fantasy named herein revealed not only the two mentioned by Rank but also another equally prominent, that of inferior parentage, which seems to lie between the two because it has a definite form and the incipient stage lacks definition. The few cases of fantasies of parentage of similar social status hint at another intermediary stage, decidedly harmonic with the concept of an outward tending libido, which might follow the inferiority stage. And the few cases of those who conceived themselves as of supernatural or very mysterious nature might point to a still higher development of the fantasy, rising to heights above mere concepts of worldly greatness as in paranoid delusions. If true this might be of much interest to those attempting to apply psychoanalytic concepts to the interpretation of the founders of religion. But the writer is far from confident that the last two are genuine stages in the development of the fantasy. The number of cases is small and they might prove to be merely circumstantially conditioned variants from the normal course. For the inferiority stage, however, in which the child thinks of himself as a foundling or poor asylum orphan, following the earlier or incipient stage in which there is no con-

cept beyond the thought of foster-childhood, there is sufficient evidence in the data presented. Of course the demonstration of a stage in the course of the fantasy characterized by inferiority concepts immediately reminds one of Adler, and it might be that ideas of greatness develop as compensations for these inferiority concepts which doubtless are accompanied by feelings of inferiority.

In considering the forms of the fantasy it is interesting to notice Rank's statement concerning the sources of the ideas of greatness. He says: "The child utilizes an accidental coincidence of actual happenings (meetings with the lord of the manor, or the proprietor of the estate, in the country; with the reigning prince, in the city. In the United States with some great statesman, millionaire)" (5, page 65). To the writer there is in that quotation the implication that in this country the fantasy is less likely to take the form of royalty or nobility. Perhaps this is so, but as a matter of fact eleven of the forty cases of expansive ideas, here studied were found to have the form of princes, kings or nobles. The appeal of non-democratic institutions is apparently not dead in the minds of American children.

As has been so often presented in the psychoanalytic literature, the principal motive of the family romance is the progressive detachment from the parents. Rank points out that revenge and retaliation motives also appear in the course of the romance, but considers them as stimuli for further imagings, further developments of the fantasy. The progressive detachment from the parents was clearly indicated in the writer's returns from those who recalled changes in conduct at the time of the fantasy. Many mentioned much thought about the matter and the deliberate seeking of proof for their notions, some wrote of depressing emotions as the consequence of their fantasy, a still smaller number state that they were moved to better behavior and greater affection for their parents (a domesticating tendency), but by far the larger number reported instigation to conduct which would inevitably alienate them from parental influence. The general direction of the conduct effect (or concomitant conduct) was clearly away from parental authority in the recollection of these adolescents. The few who mention a domesticating tendency may indicate a slight hesitation to take the plunge into independence, a slight oscillation at the outset before fixation of growth direction is established. The many reports also of revengeful and retaliatory conduct, the conscious tormenting of the parents, harmonizes perfectly with Rank's statements.

In fine this study seems to have resulted in an amplification as well as a justification of the results of psychoanalytic investigation. Psychoanalytic conclusions concerning the foster-child fantasy have stood the test of checking by a different method of research. At the same time it throws doubt upon the statement of Rank that the psychoanalysis of psychoneurotics is the only tool by which the imaginings of childhood may be studied (5. page 63). It is possible that the questionnaire might by itself never have discovered the foster-child fantasy and the family romance, but this study has demonstrated its usefulness as a tool for the checking of the results of psychoanalytic study and also for their amplification. Rank has compared psychoanalysis with empirical observation, but the questionaire as used here is not empirical observation. It approaches in its request for free personal recollection much nearer to the anamneses of psychoanalysis and has the advantage of being able to bring data readily from numbers large enough to permit of statistical treatment.

Summary and Conclusions

The foster-child fantasy has been demonstrated to be a common experience of childhood. Twenty-eight per cent of the 904 returns here tabulated could immediately recall experience with it.

Twenty five per cent of those who reported experience with the fantasy (7.8% of all) believed in it as a correspondence with facts. The others expressed many varieties of attitude from partial belief to none at all.

Three stages of development of the fantasy were indicated. In 54% the fantasy had never taken any definite form, 15% thought of themselves as poor orphans or foundlings, 18% had developed ideas of greatness. Two other stages were suggested in the returns but by numbers too small for generalization.

Suggestion (45%) and supposed mistreatment (25%) are most frequently given by those questioned as causes for the fantasy. But other causes, although less frequent, were indicated sufficiently often to deserve consideration; precocious thinking (13%), lack of physical or mental family resemblance (7%), and dissatisfaction with home conditions (5%).

The fantasy is located by more than half of those reporting in the period of 8 to 12 years of age.

The most common effect of the fantasy recalled, as described by these subjects, is conduct alienating them from parental influence and authority.

The duration of the fantasy for those who had actually believed themselves foster-children was reported to be more than a year by 49% (maximum 15 years). It was reported as of very brief duration by 24%, and as lasting a few months by 10%.

Growth and the development of intelligence is most often mentioned as the cause of the removal of the fantasy among those who had believed it, but there are two thirds as many mentions of parental intimacy and a smaller number reporting discovery of convincing proof, such as physical or mental similarities and actual records.

Comparison with the psychoanalytic presentation, especially that by Rank, results in both support and amplification of the generalizations from psychoanalysis. The frequency of the fantasy, the age location of it, causes given, two stages of its development, and the progressive detachment from the parents are alike in the results from both methods of study. The questionnaire method has, however, added the discovery of other causes, another stage in its development, some modification of statements concerning sources of the fantasy forms, and has revealed itself to be a serviceable supplement to psychoanalysis.

REFERENCES

1. ABRAHAM, KARL. Dreams and Myths. N. Y., Nervous and Mental Disease Pub., 1913. 74 pp.
2. ADLER, ALFRED. The Neurotic Constitution. N. Y., Moffat Yard, 1917. 456 pp.
3. HALL, G. STANLEY. Jesus, the Christ, in the Light of Psychology. N. Y., Doubleday, 1917. 2 Vols.
4. JELLIFFE, S. E. The Technique of Psychoanalysis. N. Y., Nervous and Mental Disease Pub., 1914. 100 pp.
5. RANK, OTTO. The Myth of the Birth of the Hero. N. Y., Nervous and Mental Disease Pub., 1914. 100 pp.
6. WHITE, W. A. The Mechanisms of Character Formation. N. Y., Macmillan, 1916. 342 pp.

The writer desires here to publicly express his appreciation of the earnest and expert assistance supplied him by the following officers of Oregon's public schools in collecting the data upon which the foregoing study is based:—C. A. Howard of Eugene, A. C. Strange of Baker, George Hug of McMinnville, A. G. Smith of Roseburg, C. R. Bowman of Klamath Falls, Naomi Williamson of La Grande, R. W. Glass of Cottage Grove, and P. E. Baker of Medford.

A METHOD OF STANDARDIZING THE COLOR VALUE OF THE DAYLIGHT ILLUMINATION OF AN OPTICS ROOM

By C. E. Ferree, G. Rand, and I. A. Haupt, Bryn Mawr College.

In a previous article (Psychol. Rev. 1912, XIX, pp. 364-373) we have given a method of standardizing the intensity of light in an optics room illuminated by daylight and have described provisions for keeping constant the intensity selected as standard. If still more exact conditions are wanted for conducting work on color sensitivity another variable, namely, the changing color of daylight, should perhaps be taken into account. It is the purpose of this paper to describe a method of standardizing the color value of the daylight illumination of a room and of correcting the changing values to the standard value in such optics rooms as we have in our laboratory.

We mean by standardizing here only a means of matching the daylight selected as desirable and of reproducing at any given time as a standard the color value which matches the daylight selected. Our plan does not include primarily a means of determining what is the most desirable color value of daylight to use, although that might well be made a valuable feature of the plan. To be of the greatest service in psychological optics the plan should be both reasonably simple of accomplishment and have a fair degree of precision. In devising a method we have endeavored to keep both of these requirements in mind. Obviously the first step to accomplish is to have a source of light the color value of which can be kept constant within acceptable limits. A well seasoned tungsten light operated by a constant current gives a light of sufficient constancy of composition to serve our purpose.[1] It has

[1] In reply to an inquiry about the constancy of color value of the light emitted by the tungsten filament operated at a constant voltage, also its constancy as compared with the light emitted by the carbon filament, we have the following from the Bureau of Standards. "With regard to the constancy of the color from lamps operated at a constant voltage, it can be said in general that almost any seasoned incandescent lamp will burn a considerable time without any appreciable change in the color of the light, unless the filament is operated at a temperature above the normal. In comparing carbon with tungsten filaments the relative constancy will depend on the conditions. If the are set at voltages which will give the same color, the tungsten will change much more slowly than the carbon In fact a tungsten filament operated at the normal color of carbon lamps will usually

the advantage too that its changes of color with change of voltage are roughly similar in direction to those which occur in daylight. A second step is to have a means of changing the color value of the light to match the daylight selected as desirable and a comparison surface on which to make the match. The first of these purposes can be accomplished by means of thin gelatine filters of known spectrum transmission, properly selected and combined to give the color values needed; and the second by means of any photometer head which presents a sufficiently good field for the comparison. We have already described a simple and inexpensive photometer for daylight work which with the proper selection of filters can also be made to serve the present purpose very well. For a description of this photometer the reader is referred to this Journal, 1916, XXVII, pp. 335-340. When the proper filters are inserted on the side next to the standard lamp this photometer can be used to make both the photometric and colorimetric comparison with no change either in the apparatus or the adjustment of the lamp. With the instrument set for the color and intensity of light selected as standard it can be determined at a glance whether the illuminaton of the room fulfills the desired requirements with regard to both of these features.

We have stated that the method employed should have a satisfactory degree of precision and sensitivity. In order to get some estimate of the sensitivity of the method or what amounts of color change can be detected at different intensities of illumination, the following experiments were conducted. (1) The light from two well seasoned tungsten lamps of equal watt value (40-watt, type B Mazda) was brought to a color and brightness match at the photometer head. The change in voltage required to produce a just noticeable change in color tone was then determined. The work was conducted in a dark room and for convenience in getting the proper range of intensities the photometer head was removed from its place in the box photometer and mounted on an ordinary photometer bar. In making the determinations one lamp, A, was operated at 107 volts and set at the desired distance from the photometer head. The other lamp, B, was operated at such a voltage and set at such a distance from the head as was needed to give an exact color and brightness match. When

burn thousands of hours with no appreciable change in color. Whether different lamps of the same kind give light of the same color depends on how closely the lamps are kept uniform in manufacture. As furnished on the market at present different tungsten lamps of the same size will be found to be more nearly alike than carbon lamps."

the match was obtained the voltage of lamp A was lowered until a just noticeable difference in color tone could be detected at the screen. A measure was thus had of the amount of color change that could be detected by the instrument in terms of the amount that is produced by a given variation of the voltage of a tungsten lamp the specification and the conditions of operation of which are known. When the voltage of lamp A was lowered the intensity as well as the color value of the light at the photometer head was changed. This required a resetting of the lamp B to bring the two photometric fields to equal brightness. However, as a check on the judgment of just noticeable difference in color tone each observation was made under more than one brightness relation between the two photometric fields. The judgment was not difficult to make. Its precision in fact compares very favorably with that of the photometric judgment. The determination was made with the lamps at different distances from the photometer head,—14, 20, 30, 40 and 50 cm. to give the differences in intensity at which it was desired to make the determinations of colorimetric sensitivity. For the sake of a comparison of the colorimetric sensitivity of the photometer heads more commonly used, the determinations were repeated with Lummer-Brodhun heads of the contrast and disappearance types. The results of these determinations are given in Table I.

TABLE I*

A COMPARISON OF THE SENSITIVITY OF DIFFERENT PHOTOMETER HEADS TO CHANGE IN COLOR OF LIGHT OF TUNGSTEN LAMP (MADZA TYPE B) OPERATED AT 107 VOLTS

	Voltage Change of Lamp to Give Just Noticeable Change of Color Tone		
Distance of Lamp From Photometer Head (cm.)	Lummer Brodhun Head Contrast Type	Lummer Brodhun Head Disappearance Type	Bunsen Head
14	0.25	0.5	1.0
20	0.50	1.0	1.5
30	0.75	1.5	2.5
40	1.00	3.0	3.0
50	1.25	3.5	3.0

*The results of this and the following tables are for Observer R. The main points were verified by a check observer, H.

(2) In the second series of experiments a comparison was made of the sensitivity to change of color tone with the three photometer heads, the Bunsen and the two types of Lummer-Brodhun, when the photometric surfaces were illuminated by the tungsten lamps, natural color, and when they were illuminated by the light from these lamps filtered through gelatines so selected as to match the daylight in one of our optics rooms at 11 A. M. on a clear day. The intensity of light at the photometer head was made the same for both kinds of illumination. The comparison was made at three intensities of illumination, corresponding to those given by the tungsten lamps in the preceding set of experiments when placed at 30, 40 and 50 cm. from the photometer head. The comparison was not made for the other intensities because they could not be obtained on our photometer bar on account of the reduction in intensity produced by passing the light through the daylight filters. That is, the lamps could not be brought nearer to the photometer head than 14 cm. and to match the tungsten lamp at 30 cm. from the head, for example, the lamp in front of which the filter was placed had to be set at a distance of 14.3 cm. The results of this comparison are given in Table II.

TABLE II

A COMPARISON OF THE SENSITIVITY OF DIFFERENT PHOTOMETER HEADS TO CHANGE IN COLOR OF LIGHT OF TUNGSTEN LAMP (MADZA TYPE B) OPERATED AT 107 VOLTS AND OF THIS LIGHT FILTERED TO MATCH DAYLIGHT

Distance of Lamp From Photometer Head Giving Equal Illumination for Each Intensity for		Voltage Change of Lamp to Give Just Noticeable Change of Color Tone					
		Type B Mazda Lamp			Type B Mazda Lamp With Daylight Filter		
Type B Mazda Lamp	Type B Mazda Lamp With Daylight Filter	Lummer Brodhun Head Contrast Type	Lummer Brodhun Head Disappearance Type	Bunsen Head	Lummer Brodhun Head Contrast Type	Lummer Brodhun Head Disappearance Type	Bunsen Head
(cm.)	(cm.)						
30	13.5	0.75	1.5	2.5	0.25	0.50	0.75
40	18.8	1.00	3.0	3.0	0.25	0.50	1.00
50	25.2	1.25	3.5	3.0	0.25	0.75	1.50

(3) A somewhat limited comparison was made of the sensitivity of the Bunsen head to change in color tone when the photometric surfaces were neutral and when they were colored. This was done because the color of some pigments is known to be very sensitive to changes in color value of the

light falling upon them. For the purpose of making this comparison a number of such standard pigments as are commonly found in psychological laboratories were substituted for the white screen of the Bunsen head. Two cases were made of this investigation. (a) Six colored screens were used, selected from the Hering series of papers: the dark red, the orange, the yellow, the yellowish green, the blue-green and the violet; and three from the Milton-Bradley series: the red-violet, the red-violet, tint No. 1 and the orange-red, tint No. 1. The light from the 40-watt lamp was in each case passed through the daylight filter referred to above. The brightness of the photometric surfaces was kept constant in all cases at a value equal to that of the white screen illuminated by the filtered light of the tungsten lamp at a distance of 74 cm. This was the highest brightness that could be obtained with this lamp for the colored screen having the lowest coefficient of reflection. This intensity of illumination was selected in order that the colorimetric comparison should be made in all cases for the same brightness of surfaces compared. The results of this comparison are given in Table III. (b) Since in the preceding

TABLE III

A COMPARISON OF THE COLORIMETRIC SENSITIVITY OF THE BUNSEN PHOTOMETER HEAD WHEN PROVIDED WITH WHITE AND COLORED FIELDS—THIS COMPARISON WAS MADE WITH THE INTENSITIES OF LIGHT ADJUSTED TO GIVE IN ALL CASES THE SAME BRIGHTNESS OF PHOTOMETER FIELDS, NAMELY, THE BRIGHTNESS OF THE BLUE PIGMENT (PIGMENT WITH LOWEST COEFFICIENT OF REFLECTION) ILLUMINATED WITH THE HIGHEST INTENSITY THAT COULD BE OBTAINED WITH THE FILTERED LIGHT OF THE MAZDA TYPE B LAMP OPERATED AT 107 VOLTS

Photometer Field	Distance of lamp (cm.) from photometer head giving brightness of colored fields equal to that of white field illuminated by this lamp with daylight filter at 74 cm.	Voltage change of lamp to give just noticeable change of color tone.
Yellowish-green	43.0	1.0
Violet	15.1	2.0
Red-violet	22.6	3.0
Orange-red, tint No. 1	40.0	3.0
Red-violet, tint No. 1	33.5	4.0
Yellow	50.8	4.0
Orange	38.0	5.0
Dark red	31.5	6.0
Blue	13.8	6.0
Blue-green	27.3	8.0
White	74.0	4.0

experiments the pigments of the higher reflection coefficients had to be used at lower illuminations than the other pigments in order to fulfill the conditions that the colorimetric comparison should be made in all cases on surfaces of equal brightness, it was decided to make a comparison of the most favorable of these colors with the neutral screen at illuminations approximately equal to that used for the color of lowest reflection coefficient in the former experiments. This, it will be remembered, was the highest that could be obtained with the filtered light of the 40-watt lamp. For this purpose the yellowish green, the yellow and orange-red, tint No. 1, screens were used. The results of this comparison are given in Table IV. These results, it scarcely need be pointed out, have a

TABLE IV

A COMPARISON OF THE COLORIMETRIC SENSITIVITY OF THE BUNSEN PHOTOMETER HEAD WHEN PROVIDED WITH WHITE AND THE MORE SENSITIVE COLORED FIELDS USED IN TABLE III, AT INTENSITIES OF ILLUMINATION EQUAL APPROXIMATELY TO THE HIGHEST THAT COULD BE OBTAINED WITH THE FILTERED LIGHT OF THE TYPE B MAZDA LAMP OPERATED AT 107 VOLTS

Photometer Field	Distance of lamp (cm.) from photometer head giving equal brightness of white and colored fields for Mazda type B lamp with daylight filter	Voltage change of lamp to give just noticeable change of color tone
Yellowish-green	16.5	0.25
White	26.5	1.50
Yellow	13.5	1.50
White	20.0	1.0
Orange-red, tint No. 1	15	1.50
White	22	1.25
Violet	15.1	2.0
White	74	4.0

much more direct bearing on the problem of selecting screens for our standardizing instrument than those of the former comparison. That is in the selection of screens for such instrument we are concerned with relative sensitivities at equal illuminations, not equal brightnesses, of screen. Obviously in the final selection of screens for any particular instrument that screen should be chosen which shows the greatest sensitivity for the range of illuminations possible for the instrument or for the range that is likely to be used.

An inspection of the results in Tables I-IV shows the following points.

(1) As might be expected smaller color changes could be detected at the higher than at the lower intensities of illumination. That is, at the higher intensities the color of the light was less saturated (the inhibitive action of the achromatic on the chromatic component of the retinal excitation) therefore a smaller change was needed to be just noticeable.

(2) The colorimetric sensitivity of the photometer heads employed is in the following order from greatest to least,—the Lummer-Brodhun, contrast type; the Lummer-Brodhun, disappearance type; the Bunsen. As bearing on the type of field that gives high colorimetric sensitivity such comparisons are of importance to colorimetry by the monochromatic method.

(3) Smaller changes of voltage were required to produce a noticeable change in the color of the light filtered to match daylight than in the color of the unfiltered light of the Mazda type B lamp.

(4) Of all the colored screens employed the yellowish-green, the violet, the yellow and the orange-red, tint No. 1, showed greater sensitivity to changes of color of light of the type produced in these experiments (changes similar to those which occur in daylight) than the white screen when the comparison was made at equal brightness of screen. When, however the comparison was made at illuminations approximately equal to the highest illumination that could be obtained with the filtered light of the 40-watt lamp only the green showed a greater colorimetric sensitivity than the neutral screen. The green screen seemed to be peculiarly sensitive to the changes which are produced by adding the longer wave-lengths to daylight. That is, the adding of the red wave-lengths appeared to decrease the saturation of the green and the adding of the yellow wave-lengths, to increase the yellow component already present. At the highest intensity of illumination used a change of only 0.25 volt was needed to cause a noticeable change in the color of the screen. Indeed at this intensity of illumination the colorimetric sensitivity of the Bunsen head was so much improved by the substitution of the green screen as to equal that of the more sensitive of the two Lummer-Brodhun heads with the white screen.

As we have already indicated the work of standardizing

for color value and intensity may be done at the same time. It may be accomplished as follows: A daylight is selected of the color value and intensity desired. On the side of the photometer head next to the standard lamp gelatine filters are inserted so chosen that when the light from this lamp run at a given voltage and set at the position on the bar needed to give the intensity match, is filtered through them, the photometric surfaces illuminated by this light match in color value the surface illuminated by the daylight. To reproduce this standard at any future time all that is needed is to reset the lamp in the same position on the bar and to reproduce the voltage. With the photometer set up at the point of work in the room or as near to it as possible, the process of checking up the illumination both as to color value and intensity becomes very simple. A glance at the photometric field is sufficient to give the desired information. Since it is not so easy to arrange for the correction of daylight to the color value chosen as standard as it is to correct the intensity, feasibility may dictate that the work be done within a certain range of variation of color value. The apparatus recommended may be used to standardize this range as follows: If the daylight chosen as desirable be that of skylight near the middle of a clear day the changes that are apt to occur during the course of the day or from day to day may be approximated roughly by lowering the voltage of the standard lamp or by lowering the voltage supplemented by the addition of one or more of the thin gelatine filters properly selected. When this is done and the lamp is reset to compensate for the change of intensity produced, a range of variation of color value is fixed within which the daylight illumination must fall or be rejected for the particular work in hand. In such a case the photometer is set so that the standard surface in the photometer head is illuminated with the light chosen as the limit towards which the color value may vary and still be accepted for the work in hand. It is very easy then at any particular time to judge whether the dominant color of the daylight incident upon the receiving surface of the photometer falls within the limiting value.

Obviously a means of correcting the changing daylight to the color value chosen as desirable would be an advantageous supplement to the work of standardizing. The method we purpose for use for this is as follows: At a distance above the diffusion sash of ground glass installed beneath the sky-

light in our optics rooms, sufficient to give a good spread of light, will be installed opaque pendant reflectors of the distributing type. These reflectors will be supplied with filters which transmit an excess of the short wave-lengths. They will be installed on separate circuits, one or more to the circuit, so that a variable proportion of the artificial light may be used as is needed. Further to vary both the composition and the amount of artificial light used, wall rheostats will be included in as many of the circuits as is found to be advisable. Quite a wide range of composition and intensity of light can be obtained also by the use of lamps of different types and wattages. The artificial units may be installed well out of the road and the desired direction and throw of light be obtained by the shape of the reflector and the angle at which it is installed. Further the amounts of daylight used in getting the desired composition may be controlled by a system of curtains placed under the skylight above the diffusion sash and the artificial units. The elaborate system of curtains which is already installed beneath the diffusion sash in our rooms will serve as they do now for fine gradations of intensity of light and may prove useful to some extent perhaps for the control of the composition.

An alternative to a flexible system of correction of actual daylight for both composition and intensity is the utilization of some one of the artificial daylights which are now on the market. Of the unfiltered sources the carbon dioxide tube gives perhaps by common agreement the closest approximation to skylight. Its cost, however, is prohibitive for the greater number of laboratories. The same thing might well be said of the best of the filter units. The ones most familiar to us all and the most available from the standpoint of cost are the blue bulb lamps. With regard to these lamps, however, only a rough approximation to daylight is claimed. We have thought that it may be of interest to show here a spectrophotometric comparison of one of them, the type C-2 Mazda lamp, and of some of the closer approximations to daylight, with the black body at 5000 degrees absolute which is sometimes taken as the standard of average daylight. (See Fig. I.) For the comperative curves given in this figure we are indebted to the Electrical Testing Laboratories, 80th street and East End avenue, New York City. The comparison here, it will be remembered, is photometric, not colorimetric.

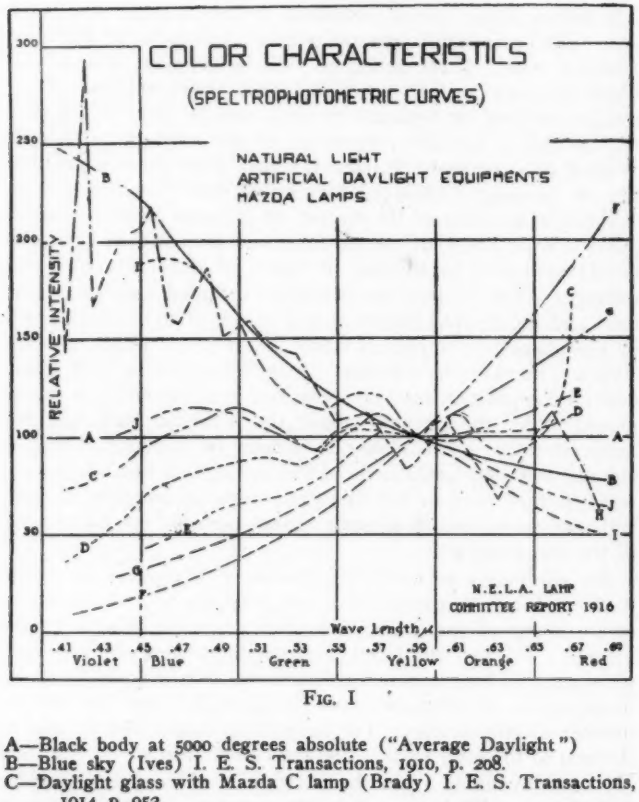

FIG. I

A—Black body at 5000 degrees absolute ("Average Daylight")
B—Blue sky (Ives) I. E. S. Transactions, 1910, p. 208.
C—Daylight glass with Mazda C lamp (Brady) I. E. S. Transactions, 1914, p. 952.
D—Bluish glass with Mazda C lamp (Sharp) I. E. S. Transactions, 1915, p. 220.
E—Mazda C lamp.
F—Mazda B lamp (7.9 lumens per watt).
G—Mazda C lamp (20 lumens per watt).
H—Moore tube (Paper read before I. E. S. November 11, 1915).
I—Trutint glass, (Luckiesh) I. E. S. Transactions, 1914, p. 839.
J—Trutint glass, (Luckiesh) I. E. S. Transactions, 1914, p. 839.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

Communicated by E. B. TITCHENER, H. P. WELD and K. M. DALLENBACH

XLVII. SIZE *vs.* INTENSITY AS A DETERMINANT OF ATTENTION

By ALMA M. BOWMAN

This paper is related to two of the experimental studies on the determination of attention which have been published from the Cornell Laboratory. Meads, working with form *vs.* intensity, obtained from five observers the positive result "that a light-form of relatively low intensity may have as great a power to attract attention as a formless light of relatively high intensity."[1] Curtis and Foster, working with size *vs.* intensity, were unable to reach any positive conclusion. Their three observers gave widely differing results: one showed a positive and one a slightly negative effect of size, while to the third size was apparently indifferent; and in the case in which the effect of size was negative, spatial position proved to be more important than a four-fold increase in area.[2]

It may very well be the case that form is, and size is not, a powerful determinant of attention. If this conclusion is sound, then the diversity of result obtained by Curtis and Foster means simply that in their experiment the effect of size was cut across by other influences which the plan of the experiment did not permit them to bring under control. There is, however, between the two studies a difference in method. Meads employed the method of limits; Curtis and Foster, taking their cue from the report of one of Meads' observers (Professor Weld) that expectation might be a disturbing influence, changed over to the method of constant stimulus-differences.[3] Since now the method of limits has given positive results, and the method of constant stimulus-differences has given results that are positive, negative and indifferent, it seems plainly necessary to attack the problem of size by the method of limits. This is what we have done in the present paper.

Apparatus.

The apparatus was set up, as usual, in a dark room. The observer, screened by heavy black curtains, sat comfortably at a table, his head held firmly in position by an adjustable biting board. Facing him at the distance of approximately 50 cm. was the ground glass plate upon which the two areas and the fixation-point appeared. The stimuli were as before: a standard Greek cross of 56 sq. cm. was compared with similar crosses of half and twice its area. The fixation-point lay between the crosses, at a distance of 10 cm. from each; it was illuminated throughout the experiment, though it was not visible during the exposure of the crosses.

[1] L. G. Meads, "Form *vs.* Intensity as a Determinant of Attention," this JOURNAL, xxvi., 1915, 151.

[2] J. N. Curtis and W. S. Foster, "Size *vs.* Intensity as a Determinant of Attention," this JOURNAL, xxviii., 1917, 294f.

[3] Meads, 151; Curtis and Foster, 293.

Exposure was made by a Wundt gravity tachistoscope set for one-tenth of a second; the actual time, as measured by the Hipp chronoscope, was 100.3 ± 0.7 sigma. Heavy felt pads, laid at the base of the instrument to catch the shutter, made the exposure almost noiseless.[4]

The light was obtained from a 40 watt Mazda Daylight lamp placed in a projection lantern behind the tachistoscope. The rays passed into a diffusion box which was white inside, and whose far end contained two circular ground glass windows, 18 cm. apart and 8 cm. in diameter. The amount of light issuing from these windows was controlled by cardboard shutters, which by a graded series of circular openings carried the illumination by steps of 4% down to 12%, by steps of 2% down to 4%, and by steps of 0.5% down to 0.5%. The illumination of the comparison crosses might be varied within these limits; that of the standard cross remained constant at 20%.[5]

Experiment.

The following instructions, identical with those of the previous work, were read to the observers at the beginning of every experimental hour. "At the signal 'Now' put your attention definitely on the fixation-point. Two crosses of unequal size will be exposed. Judge which of them, if either, is the more clear; that is, which of them catches your attention the more." The method employed, for the reasons given above, was the method of limits. The observers judged 'Right,' 'Left,' or 'Equal.' 'Doubtful' judgments, in accordance with present practice, were thrown out, and the observations were repeated.[6]

The observers were F. L. Dimmick (*D*) and L. B. Hoisington (*H*), assistants in psychology; Myrl Cowdrick (*C*), graduate student in the department; and K. M. Dallenbach (*Da*). *D*, *H*, and *Da* were highly practised in the observation of clearness.[7]

Before the experiment began, a preliminary series was given to every observer in order to determine the critical range. Short warming-up series were also given at the beginning of every experimental hour. The experimental series were arranged for measurement of the space error and for compensation of the irregular influences of practice and fatigue.

Every observer made four series: two with the large cross, standard right and left; and two with the small cross under similar conditions. The series was composed of ten ascending and ten descending observations. The order in which the four series were presented to the observers was as follows:

---

[4] Meads (150) used a spring tachistoscope with a time of 110 ± 8 sigma; Curtis and Foster (293) used a pendulum tachistoscope with a time of 110 ± 3 sigma.

[5] Meads and Curtis and Foster used an episcotister; but Curtis and Foster found the range of experimental variation insufficient (294). Our arrangement overcomes this difficulty.

[6] S. S. George, "Attitude in Relation to the Psychophysical Judgment," this JOURNAL, xxviii., 1917, 33ff.

[7] *D* is the observer who showed the positive effect of size in the work of Curtis and Foster (293, 295); *H* had observed in a study of the measurement of attention (this JOURNAL, xxvii., 1916, 459); and *Da* had observed in several such studies (this JOURNAL, xxiv., 1913, 465ff.; xxvii., 1916, 443ff.; xxix., 1918, 204ff.).

Series	Observers			
	D	*H*	*Da*	*C*
Large cross, standard right	1	2	3	4
Large cross, standard left	2	3	4	1
Small cross, standard right	3	4	1	2
Small cross, standard left	4	1	2	3

A series was completed within a single experimental hour.

Results.—The results are given in the table below:

O	Position	Small Cross				Large Cross			
		Av.	M. V.	Gen'l Av.	Deviation from stan'd	Av	M. V.	Gen'l Av.	Devia'n from Stand'd
D	right	53.0	13.8	43.2	+23.2	10.9	4.7	10.5	—9.5
	left	33.4	8.3			10.1	4.7		
H	right	79.2	4.8	59.1	+39.1	27.6	5.8	15.2	—4.1
	left	39.0	10.6			2.9	1.5		
Da	right	40.2	6.6	38.9	+18.9	18.0	2.5	14.0	—6.0
	left	37.6	5.2			10.1	4.3		
C	right	30.6	5.2	24.2	+4.2	23.0	5.6	19.0	—1.0
	left	17.7	3.1			15.1	3.7		

This table shows the average per cent. of illumination necessary for the comparison crosses if they are to be equal in clearness to the standard cross. The data of the 5th and 6th, 9th and 10th columns indicate that the small cross, to be as clear as the standard, must be more intense; and that the large cross, to be as clear as the standard, must be less intense. The *direction* of our results is thus constant for all four observers. There is, however, a marked difference between *C* and the other observers: her *plus* and *minus* deviations from the standard are very small. *C*, it may be noted, was a comparatively untrained observer. If we consider only the results of the three observers who were highly trained in the observation of clearness, we find uniformity of tendency, well-marked in all cases, though showing individual variation of the order 2 to 1. For *C*, the *m. v.* is also larger with both crosses than the difference between the standard and comparison stimuli. Even for the trained observers, the *m. v.* with the large cross is not always satisfactory.

But although the direction of the results is constant, the *magnitude of the space error* proves that the effect of size was cut across and obscured by another influence. The data which indicate the effect of position are given in the 3d and 7th columns of the table. For both of the comparison areas and for all observers the left-hand position is the more favored. The position-ratio is 2 to 1 for *H* and *C* with the small cross, and rises as high as 9 to 1 for *H* with the large cross. The left-hand position has, however, no advantage in the matter of relative variation.

Conclusions.—We conclude that the difference between the results obtained by Meads for form and by Curtis and Foster for size is a true difference, not attributable to the difference of the methods employed.[8]

If we may judge by the results of our trained observers, size as determinant of attention may bear to intensity a ratio which varies from 3:1 to 4:3. For a less trained observer size was indifferent.

In the two experiments upon size (that of Curtis and Foster and our own) spatial position proved to be a major disturbing influence. It seems that the left-hand position has an attentional advantage. We are now undertaking a further and more systematic study of this subject.

[8] Cf. also H. Woodrow, "Outline as a Condition of Attention," *Journal Exp. Psychology*, i, 1916, 23 ff.

THE TONOSCOPE AS A MEANS FOR REGISTERING COMBINATION TONES[1]

By Evelyn Gough and Genevieve Robison

The possibility of producing and demonstrating combination tones in the mass of air was clearly established by Helmholtz.[2] He found that the essential condition for the generation of strong objective combination tones was that the same mass of air should be violently agitated by two generating tones simultaneously. The siren and the harmonium, each of which possesses a common wind chest in which the air is set in vibration by all notes played upon it, produced objective combination tones which were extremely powerful.

The presence of the combination tones in the air was demonstrated by the use of vibrating membranes and resonators adjusted to be in unison with these tones. Helmholtz found in using the harmonium that when the two generators were supplied by air from different bellows the objective part which the resonators reinforced was much weaker. When no common air chamber or mechanical connection existed for the two tones, as in the case of tones from two singers, two separate wind instruments, or two violins, the reinforcement of the combination tones by resonators was small and dubious. In such cases the combination tones, which might be clearly audible, he believed to originate, from vibrations in the ear, either in the tympanic membrane or in the ossicles of the middle ear.

Apparatus and Method

The present paper reports experiments showing that the Seashore tonoscope has certain unique advantages as an apparatus for the objective demonstration of combination tones.[3] A description of the apparatus employed may throw light on the results. An early model of the tonoscope driven by a synchronous motor at a speed regulated by a 10 v. d. tuning fork was available. It was energized by a commercial alternating current. For controlling the strength of current closely the rheostat furnished with the tonoscope was supplemented by electric lamps. A manometric capsule, designed with special view to effectiveness with the high pitches of female voices, was substituted for the standard piece. It differs from the standard in certain respects. The capsule is much smaller, the membrane being 12.5 mm. in diameter. The gas supply is admitted at relatively high pressure through a small inlet (.25 mm. in diameter). A relatively large tube (2 mm. in diameter) leads from the gas chamber to the flame tip. The flame tip is one of the small-sized tips which are used on small acetylene hand lights. A gas cock makes possible fine adjustments of the flame.

[1] From the Psychological Laboratory of Smith College. The writers wish to express their indebtedness to Professor D. C. Rogers for his generous assistance in regulating and modifying parts of the apparatus used and for his helpful suggestions in the preparation of this paper.

[2] Helmholtz, Sensations of Tone, pp. 152-159.

[3] Seashore, The Tonoscope, Psychol. Monog., No. 69, 1914, XVI, 1-12.

Ordinary dental dam is used for the membrane. For the purpose of this particular experiment a rubber voice tube 30 cm. long was employed. It was stopped at the end with a cork and sealed. In opposite sides of this tube were inserted two smaller tubes with tin funnels at their free ends for mouth-pieces.

Two tones sung simultaneously into the branches of the voice tube evidently move along a common air passage to the membrane and through it are communicated to a gas chamber and a flickering flame in turn. In passage they strongly agitate a common air chamber, a common gas chamber, the rubber walls of a common voice tube, and a loosely stretched membrane, any one of which would, perhaps, be capable of adding combination tones to two primary tones which are made to vibrate through it.

The purpose of this experiment has been to discover whether or not with this apparatus it is possible to get distinct readings on the tonoscope for vibration rates of tones formed by the combination of two tones sung simultaneously into the voice tube, which are wholly different from the readings produced by either tone sung alone.

The inquiry has so far been confined to difference tones of the first order and summation tones of the first order. Pairs of pitches were selected which could be sung easily and read plainly from the tonoscope, and whose difference or summation tones, if they appeared would also possess vibration rates within the effective range of this instrument. Variously pitched tuning forks gave the objective pitches which the singers attempted to duplicate at each trial. Each of the two tones was sung and recorded separately, then they were sung together, and a complete reading was taken.

RESULTS

			Pitch from Tuning Fork	Tonoscope Readings
1	Generators	a^1	440 v.d.	146 x 3 = 438 v.d.
				219 x 2 = 438 v.d.
		c^1	264 v.d.	132 x 2 = 264 v.d.
	Difference Tone	f		174 x 1 = 174 v.d.
2	Generators	g^1	396 v.d.	196 x 2 = 392 v.d.
				131 x 3 = 393 v.d.
		b	247½ v.d.	125 x 2 = 250 v.d.
	Difference Tone	d		142 x 1 = 142 v.d.
3	Generators	g^1	396 v.d.	192 x 2 = 384 v.d.
				128 x 3 = 384 v.d.
		a	220 v.d.	110 x 2 = 220 v.d.
	Difference Tone	f		164 x 1 = 164 v.d.
4	Generators	f^1	352 v.d.	175 x 2 = 350 v.d.
				117 x 3 = 351 v.d.
		a	220 v.d.	110 x 2 = 220 v.d.
	Difference Tone	c		131 x 1 = 131 v.d.
5	Generators	b	247½ v.d.	124 x 2 = 248 v.d.
		g	198 v.d.	196 x 1 = 196 v.d.

RESULTS—Continued

		Pitch from Tuning Fork	Tonoscope Readings
Summation Tone	(a^1+)		(Theoretical position at 148 x 3 = 444 v.d.) None observed
6 Generators	a	220 v.d.	114 x 2 = 228 v.d.
	f	176 v.d.	171 x 1 = 171 v.d.
Summation Tone	(g^1)		(Theoretical position at 199 x 2 = 398 v.d. 133 x 3 = 399 v.d.) None observed

With the manometric capsule which was employed in these tests tones whose vibration rates lie within a range of the first, second, and third, multiples of the dot frequencies on the tonoscope are easily read. The patterns corresponding to the different dot frequencies, however, are distinctly different in appearance. Pitches which coincide in rate with a dot frequency within the range of the tonoscope give strong contrasts and bring out the dots in much elongated form and at the same distances apart that they hold on the stationary drum. Those which correspond to multiples of the dot frequencies cause the dots to appear with degree of contrast, separation and elongation which are in inverse proportion to the multiple numbers.

Readings for the difference tones were easily gotten in the tests, and where the generator tones were so chosen as to give difference tones within the range for direct reading, the dots representing these tones stood out with striking clearness. In fact, where the generator tones corresponded to multiples of dot frequencies while the difference tones coincided with them in rate, as in the four illustrations given in the table, the dots representing the difference tones were generally clearer than those representing the generator tones. The correspondence between the tonoscope readings for the generator tones and the vibration numbers engraved on the forks from which the pitches were taken was only approximate. The readings for the difference tones, however, coincide exactly with the calculated differences between the readings for the generators.

The results from the summation tones were entirely negative, no readings corresponding to the summation numbers appearing on the screen, although strong primary tones whose summation effect could have been recorded if sufficiently pronounced were used. Modifications in the apparatus favoring the higher tones or the use of lower primary tones might possibly lead to a different outcome.

In summary: The Seashore tonoscope furnishes a means for the demonstration of difference tones which possesses the following distinct merits. The apparatus furnishes conditions favorable for the generation of strong objective difference tones. Clear visible evidence of the difference tones is present. Objective visible readings are produced for each primary tone separately, and for the difference tone and both generating tones simultaneously. A practically untrained observer can record readings for the three tones accurate to one or two vibrations a second.

References:

1. Helmholtz, H. L. F. Sensations of Tone. Tr. by A. J. Ellis, 1895.
2. Seashore, C. E. The Tonoscope. Psychol. Monog., No. 69, 1914, XVI, 1-12.

BOOK REVIEWS

The Elementary Nervous System. By G. H. PARKER. Philadelphia, J. B. Lippincott Co., 1919. 229 p.

This interesting volume is one of the series of monographs on Experimental Biology which are being prepared by a number of American biologists. Its purpose, according to the author, is to portray by means of experimental and quantitative methods the elementary nervous system as it exists in the simpler animals and in the simpler parts of the more complex forms.

In the introduction a brief comparison is made between the ancient and modern ideas of the nervous system wherein it is shown that the complex mechanism of higher animals, according to modern research, is composed of relatively simple elements, the neurones, arranged upon a comparatively uniform plan which from a physiological standpoint falls into three distinct categories. The first is a *receptor* system which is found in the sense organs that receive the external stimuli and produce the sensory impulses. The second is an *adjustor* system or the central nervous organ that receives the stimuli from the receptors and directs them to the appropriate muscles, glands, etc., for an orderly response. In the more highly organized animals the impressions may be made upon the adjustor system as memories and become a permanent part of the animal's nervous equipment. The third category consists of the *effectors* such as muscles, glands, etc., which enable an animal to react to changes in its environment. These three categories are not found in the nervous system of the lower forms but are shown to have developed from a simple, primitive condition consisting of nothing but an effector system.

The main body of the book is divided into three sections. The first one deals with experiments on the behavior of sponges where no nerve tissue is found. Movements are accomplished by means of cells comparable to a primitive form of smooth muscle fiber or others that represent a stage of differentiation between amoeboid motion and simple muscle contraction. No coordinating system is present anatomically or physiologically. All the evidence collected shows that sponges contain the beginnings of a neuromuscular mechanism with the most primitive constituent, muscle, around which the remainder of the system is supposed to have been evolved. Independent effectors, similar to those found in sponges which are not innervated but are activated by direct stimulation, are shown to occur in the higher animals. The examples cited are the iris of the eye, heart muscle, amnion of embryo chicks and acontia of sea anemones. Also sluggish forms of transmission exhibited by the ordinary tissues of the sponge are found in the more complex organisms as in their ciliated epithelium.

The second section of the book is concerned with receptor-effector systems that mark the second step in the evolution of the nervous system. In the sea anemone the neuromuscular structure is composed of a dense meshwork of neurofibrils extending from the outer body layer to the more important systems of muscles in the mesenteries. In these animals the effector systems are more numerous and complicated than in the sponges. They consist of mucous glands, ciliated

epithelia, nematocysts and muscles. Of these, however, the first three are without nervous influence and are, therefore, independent effectors. In the jellyfishes a marked increase in sensitivity is found over that manifested in sponges due to the acquirement of sense organs or receptors and the establishment of nerve connections that create a quicker and more efficient system by increasing the sensitiveness of the new member, the receptor, rather than on a considerable change in the original member, the effector. From the anatomical standpoint this primitive nervous system as seen in the Coelenterates is diffuse, in the form of a nerve net, but it shows some polarity by forming bands or trunks with some resemblance to nerves that permit receptive cells to control muscles or effectors situated some distance from them. Physiologically, the parts are relatively independent with no central control.

In the third section of the book the author discusses the anatomical and physiological relation of the elementary nervous system of the sponges and Coelenterates to the central nervous system of the more complex animals where a new element arises in the form of a central organ or adjustor in the region between the receptor and effector. It is this portion that, in the higher animals and man, has given rise to the central nervous system with all its complexity. In conclusion he says, "Thus the system that arose secondarily around an independent effector, muscle, has in the end gained such supremacy as to take to itself a number of independent effectors, any one of which might in the beginning have served as the nucleus around which the first nervous tissue could have taken origin."

The argument is based upon minute anatomical evidence and a great mass of data collected from numerous ingenious experiments, most of which are very simple in nature but at the same time furnish evidence that leads to definite conclusions. The subject is presented with unusual clearness and accuracy of detail and in such a simple and interesting manner that persons with no more than an elementary training in biology can read this monograph with profit. It is a contribution of utmost importance to the origin and evolution of the nervous system.

I. A. FIELD.

El Psicoanalisis. By HONORARIO F. DELGADO. Lima, 1919. 58 p. Also *La Psicologia de la Locura.* Madrid, 1919. 20 p.

Dr. Delgado's monograph on psychoanalysis is very inclusive and shows a thorough theoretic knowledge of the subject. His reading of both German and English literature has obviously been very extensive, and has not been limited to any one school, so that in his own synthetic presentation, although he follows Freud very closely, he does not neglect the contributions of such men as Adler, Jung, Maeder et als.

The first chapter of Dr. Delgado's *El Psicoanalisis* is devoted to an outline of the Freudian views of the rôle of the sex instinct in human life. Broadly speaking, it deals with the subjects of infantile sexuality, narcissism, Oedipus and Electra complexes, arrest of the libido, repression, the dream mechanism, symbolisation, etc. The application of these principles in the explanation of such perversions as homosexuality is adequately set forth, and the necessity of a natural development of the libido for the formation of a normal personality is emphasized.

The second chapter of the monograph is a brief but clear presentation of the theory of the sexual etiology of the psychoneuroses and allied mental diseases. The anxiety neurosis, neurasthenia, hysteria, compulsive neuroses, paranoia, dementia praecox, and manic-depres-

sive psychoses receive separate consideration in a manner which shows that the author has grasped the full implications of the psychoanalytic doctrines. A little peculiar, however, may appear the inclusion in the same category of the *defence mechanism,* which is usually considered with the other Freudian mechanisms described in the preceding chapter.

The third chapter gives an account of the methodology and technique of the psychoanalytic therapy. The last two chapters are of a more general nature, and are devoted to a defence of the psychoanalytic theory and practice, and to a rather philosophical discussion of the new dynamic conception of the psychic life which the psychoanalytic statements involve.

The small pamphlet called *La Psicologia de la Locura* (The Psychology of Madness), is a brief statement of the new psychological data concerning this problem which the psychoanalysts have given us. It is written in popular style, and does not pretend to have the scope of the larger monograph. The latter is to be highly recommended to students best acquainted with the Spanish language both as a summary of the subject and as a guide to other literature in the same field.

PHYLLIS BLANCHARD.

BOOK NOTES

The Psychology of Musical Talent. By CARL EMIL SEASHORE. Boston, Silver, Burdett and Co., 1919. 288 p.

This comprehensive monograph is addressed to students of applied psychology. Its content must appeal to all interested in music, and teachers and students of childhood should also find much material of value in it. For years the author has devoted himself to this subject, and he is our chief American authority upon it, so that psychology may well congratulate itself that it has here the carefully drawn-up statement of the results of long and constant study.

After treating of the psychology of the musical mind the author then discusses in subsequent chapters the sense of pitch, of intensity, time, rhythm, timbre, consonance, auditory space, voluntary motor control, musical action, musical imagery and imagination; musical memory, intellect, and feeling; and the individual and training in the art.

Human Psychology. By HOWARD C. WARREN. Boston, Houghton Mifflin Co., 1919. 460 p.

This book has no preface or introduction. It has a long list of acknowledgments. It is evidently the material of the author's course of instruction well digested by many years of active teaching. It is abundantly illustrated and has a great many tables. The following are the chief chapters treated: The Science of Psychology; The Organism; The Neuro-Terminal Mechanism; Physiology of the Neuron; Stimulation, Adjustment, and Response; Behavior; Conscious Experience; The Senses; The Components of Mental States; Primary and Secondary Mental States; Succession of Mental States; Attitudes; Character and Personality; Organized Mental Life; with an Appendix on The Mind-Body Relation, Mechanism and Purpose, Neural Activity, and The Visual Process.

The Psychology of Nationality and Internationalism. By W. B. PILLSBURY. N. Y., D. Appleton and Co., 1919. 314 p.

The writer's work is more or less suggested by Graham Wallas, and his position is a compromise between that of MacDougall, with his insistence on immediate instinct, and that of Trotter, who explains all social phenomena by the fear of the individual for the social whole, with the consequent dominance of convention. The chief chapters are: The Problem of Nationality; The Nation as a Psychological Unit—Social Instincts; Hate as a Social Force; Nationality in History; Nationality in the Process of Naturalization; The Nation and the Mob Consciousness; The National Mind; The Nation as Ideal; Nationality and the State; and Nationality and Super-Nationality as Expressed in a League of Nations.

An Outline of Abnormal Psychology. By JAMES WINFRED BRIDGES. Columbus, R. G. Adams and Co., 1919. 126 p.

Nothing could be more admirable, timely, or needed than an outline of this kind. Perhaps no two people would have made the same

tabulation and classification of subject matter, and some would take exception even to his division of it into three parts: (1) The Abnormal Phenomena; (2) The Mental Syndromes or Symptom-Complexes of Insanity; and (3) The Borderline Diseases: Psychoneuroses and Epilepsies. It would be also easy to quarrel with his bibliography, which indicates that he is warped a little by his partisanship for introspection on the one hand and that he is not acquainted with much of the best recent German literature on some of the topics. This perhaps is the chief defect of such an outline. But after all this is said, while the author's classification of abnormal phenomena is not that to which most adhere now, everyone ought to be thankful for this handbook, which is so timely and so conscientiously wrought out.

Sinnesphysiologische Untersuchungen. By JULIUS PIKLER. Leipzig, Johann Ambrosius Barth, 1917. 515 p.

Here we have an admirable and comprehensive survey of the results of the experiments in sense physiology up to the present time with an attempt to synthetize and balance accounts as to what is known and unknown, valuable and otherwise. It is a work of great value, and we hope to have it more fully reviewed at some future time in these pages.

The Voyage of a Vice-Chancellor. By ARTHUR EVERETT SHIPLEY. N. Y., G. P. Putnam's Sons, 1919.

These are extracts from a private diary the author wrote while on an extensive tour of the United States in 1918 as a member of the British University Mission. It is difficult to see any *raison d'être* for such a book. Most of it consists of jottings calendar-wise, giving crude impressions of his trip to this country, while a chapter ambitiously headed "University Education in the United States" gives the crudest and most superficial of all views. To not a few American educators upon whom the Vice-Chancellor made any impression at all he seemed to carry an English atmosphere so dense and impervious as to make him almost inaccessible to new impressions, a man who loved to talk and did so with an English accent delightful to many, but tedious and prolix. It is significant and ominous to our minds that this much-heralded British commission should have devoted so much time to matters so entirely irrelevant to the purpose of the commission, although, so far as we can see, this trivial book may be one of the chief results of the pedagogic expedition.

The Field of Philosophy. By JOSEPH A. LEIGHTON. Columbus, R. G. Adams and Co., 1919. 485 p. (Second revised and enlarged edition.)

This work, the publisher tells us, "gives a concise, clear and comprehensive account of the principal views of the nature of the world and the human soul, and of the meaning of thought and human life, advanced by thinkers in the chief ages of European and American thought down to the present time." Although comprehensive itself, it "furnishes ample guidance for further study."

The first part, which is historical, is rather full, down to and including Scholasticism, with which it stops. Part II consists of the problems of modern philosophy, with a chapter to each of the following: Reality, Dualism, Materialism, Kant, Spiritualism or Idealism, The One and the Many, Evolution and Teleology, The Self, Concepts of Metaphysics, Epistemology, Criteria of Truth, Values, Philosophy

of History, Philosophic Progress and Discipline, and Current Issues in Regard to Consciousness, Intelligence, and Reality.

The Health of the Teacher. By WILLIAM ESTABROOK CHANCELLOR. Chicago, Forbes and Co., 1919. 307 p.

The purpose of this book is to guide teachers in the care of their own health while teaching, which is a serious problem. It means increased strength and efficiency for the day's work, overcoming tendencies to disease, increasing the joy of life, and postponing death. The thirty-eight chapters are divided into parts: (1) Principles of Diagnosis and Cases, and (2) The Rationale of Health Control. Part I includes chapters on The Instinct to be Well, Differential Diagnosis, and on eighteen different troubles with case illustrations of each. Part II is on sleep, diet, exercise, etc., and on the care of the different parts, organs, and functions.

Brightness and Dullness in Children. By HERBERT WOODROW. Philadelphia, J. B. Lippincott Co., 1919. 313 p.

The chief chapters are: The Measurement of Intelligence; Brightness and Dullness; Brains; Physical Defects; Anatomical Age; Pedagogical Age; Simple Mental Processes; Association, Memory, and Attention; Complex Mental Processes; Mental Organization; Heredity; The Organization of Education; and Educational Methods. This book is in the Lippincott Educational Guides series edited by W. F. Russell, Ph.D.

Etude-Critique sur L'Evolution des Idées Relatives à la Nature des Hallucinations Vraies. By RAOUL MOURGUE. Paris, Jouve et Cie., 1919. 66 p.

This somewhat voluminous author writes here rather systematically his conclusions, based largely upon the French writers, that there is great difference in the degree of the hallucinated image and that this is favored by every anatomical reference. He is evidently a rather extreme disciple of Moreau in believing that crepuscular states play a great rôle here. The discussion, on the whole, does not bring us into very close relation with the subject; nor does it suggest much that is new.

Aces for Industry. By RUFUS STEELE. Boston, Houghton-Mifflin Co. 93 p.

This is an indexless, chapterless, and so far as we can see rather aimless but sprightly, gossipy booklet with considerable conversation and plenty of idealism, a book about which it is impossible to form any opinion without reading it all; and the reviewer, after many attempts, can find no incentive to do this. The publisher calls it "an absorbing story of a big new idea in American life," viz., that the chief need of industry is workers with the spirit of the aviator, not content to do a day's work in routine fashion but eager to improve everything.

Aesthetics: A Critical Theory of Art. By HENRY G. HARTMAN. Columbus, R. G. Adams and Co., 1919. 250 p.

The writer here attempts to answer the question, what are the elements of individual interest in painting, music, and poetry? He does not aim so much to increase our appreciation as to correct the old methods of determining substance, origin, and value of art. He does

not deem it unitary, and thinks the psychological or subjective factors have been forced into one-sided prominence, so that our art psychology needs reconstruction. The chapters are the following: Beauty and Art; Art and Theory; What is Art? The Substance of Art; Formulas and Methods in Art-Theory; Painting; Poetry; and Music.

Business Inspirations. By CHRISTIAN D. LARSON. N. Y., Thomas Y. Crowell Co., 1919. 163 p.

Of the twenty-seven chapters of this book the typical ones are headed: Follow the High Vision, The Magic of Sincerity, It Pays to Look Well, What Makes Men Great, Giving Your Whole Self, How to Work, Creating the Right Idea, The Will to Do More. It is written in a sort of hortatory way and is related to other books on business somewhat as the work of a good revivalist is related to that of a stated pastor.

Proceedings of the American Medico-Psychological Society. Vol. XXV, June, 1918. Baltimore, American Medico-Psychological Assn., 1918. 397 p.

Prehistoric Villages, Castles, and Towers of Southwestern Colorado. By J. WALTER FEWKES. (Bureau of American Ethnology, Bulletin 70.) Wash., Govt. Printing Office, 1919. 79 p. (33 plates.)

Handbook of Aboriginal American Antiquities. By WILLIAM H. HOLMES. (Bureau of American Ethnology, Bulletin 60, Part I.) Wash., Govt. Printing Office, 1919. 380 p.

PRIZE IN PSYCHOPHYSICS

The prize of $100 offered in 1914 for the best paper on the Availability of Pearson's Formulae for Psychophysics (this JOURNAL, XXV., 148), has been awarded to Dr. Godfrey H. Thomson, Armstrong College, Newcastle-upon-Tyne, for a paper entitled "On the Application of Pearson's Methods of Curve-Fitting to the Problems of Psychophysics, especially to the Data of Urban's Experiments on Lifted Weights: in four parts, together with Part V. On the Use of Compound Curves in the Analysis of Heterogeneous Material, and Part VI. On an Outline of an Attempt to Make a Generalised Psychometric Function."